KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, FEBRUARY 17, 1882.

CONTENTS OF No. 16.

	PAGE.
Birth of the Moon by Tidal Evolution. By Dr. Ball, Astronomer-Royal for Ireland. Part I.	331
Microscopic Vision and Minute Life. By H. J. Slack, F.G.S., F.R.M.S.	332
The Last Transit of Venus. By the Editor	333
The Crystal Palace Electrical Exhibition. Second Notice. (*Illust.*)	335
Popular Astronomy by the Chief of a Great National Observatory	336
REVIEWS: Sir Edmund Beckett on the Laws of Nature—The Food we Eat—Landauer's Blowpipe Analysis—Science for All	339
Night Minima of Algol. 1882	339
The Menacing Comet. By the Editor. (*Illustrated*)	340
The Effects of Tobacco.—Part II. By Dr. Muir Howie	342
How Spiders Fly	343
Intelligence of Dogs	344
CORRESPONDENCE: Optical Illusion.—Cat's Eye Timepiece—Weather Forecast, &c.	344
Queries	346
Replies to Queries	346
Answers to Correspondents	347
Notes on Art and Science	348
Our Mathematical Column	348
Our Whist Column	349
Our Chess Column	350

BIRTH OF THE MOON

BY TIDAL EVOLUTION.

BY DR. BALL, ASTRONOMER-ROYAL FOR IRELAND.

PART I.

THE daily rise and fall of the sea, which we call the tide, has long been known to be connected with the moon. The discovery of the law of gravitation enabled Newton to explain how the tides were caused. Newton showed that the tides were partly due to the attraction of the sun, but chiefly to the attraction of the moon.

In the present paper we shall principally consider the tides which are produced by the moon, and the reader will please understand that this is the tide to which we refer, except otherwise stated. The tides are of the utmost importance in our seaports. They are not, perhaps, very often employed to do useful work, in the sense of driving machinery, but on work of one kind or another the tides are unceasingly busy. No one who has watched the rise and fall of the tide on the beach, or the currents of the tide in a river, can doubt that the tides do work. We need not here attempt to enumerate all the varieties of tidal work. Let it be sufficient to mention one kind, as an illustration. The waters of a strong tidal river like the Avon, at Bristol, are heavily charged with mud in suspension. The tides are carrying that mud, and in doing so, they accomplish work, at an expenditure of energy which could be expressed by an equivalent amount of horse-power.

The steam-engine will only yield an appropriate horse-power when the boilers are heated by a proportionate quantity of fuel. So also the tides can only accomplish their gigantic work all over the world because they are bountifully fed with energy. Whence do the tides obtain their energy? They draw it from a certain store which is being steadily squandered and never replaced. The supply in the store may be great, but it is not inexhaustible. It is easy to discover the store when we consider the circumstances of the case. Fixing our attention solely upon the earth and the moon, we can enumerate the different forms of energy which may conceivably be available. The case can be very simply stated; there is a store of energy in the earth due to the fact that the earth is rotating on its axis. There is a similar store of energy due to the rotation of the moon on its axis. The latter is, however, very small, and may be left out of sight for the present. A third source of energy is due to the fact that the moon is separated from the earth, and that, as it would require energy to force the earth and the moon asunder, so, if the earth and the moon were allowed to draw together, energy would be given out. To this must be added the energy due to the motion of the moon in its path around the earth. To put the matter briefly, we may say that the available sources of energy for the tidal work must be sought either in the rotation of the earth on its axis, or in the distance of the moon, including in the latter case the energy due to the velocity of the moon in its path, which is intimately connected with the distance of the moon from the earth. As the tides are incessantly drawing on this store, it is imperatively necessary that one or both of these sources of energy be decreasing; we are therefore forced to admit that the velocity of the earth's rotation on its axis must be diminishing, or that the distance of the moon is decreasing, or that both velocity and distance are decreasing. There can be no doubt as to which is the true explanation, for the question is determined by a well-known dynamical principle. This principle assures us that the supply of energy required by the tides must be drawn from the rotation of the earth. Indeed, we may go further than this. It is most curious to observe that a second draft is made upon the reserve energy stored up in the earth's rotation; this second draft is actually expended in pushing the moon away from the earth.

We have, then, two very remarkable astronomical consequences of the tides. These consequences are founded on dynamic principles, but in a manner not very easy to explain without going into technical matters. The first consequence is that the velocity with which the earth rotates must be abating—in other words, that the tides on the earth are increasing the length of the day. The other consequence is not a little remarkable. It states that the moon must be describing an orbit around the earth, which, in the course of ages, is gradually becoming larger and larger. It must be remembered that these two consequences of the tides are not mere speculations. They are as true as the laws of dynamics, which have been confirmed by universal experience. The propositions just stated will not be questioned for a moment by anyone who is acquainted with mechanical principles. Let us take first the important fact that the length of the day is gradually increasing. It must be admitted that the change in the length of the day is excessively slow. Even in centuries, the change is but a fraction of a second; but the change is always in one direction, and, consequently, ever since the earth and the moon commenced to have a separate existence, the length of the day has been getting steadily greater and greater, until it has at present attained the well-known 24 hours. We are now to look back into the history of the earth and the moon in very remote antiquity. Our ordinary chronologies of thousands of years are here quite inadequate. The unit of time adapted for the earth-moon history is one million of years. A million years ago the length of the day was appreciably shorter than it is at present. There was a time when the day, instead of being 24 hours, was only 23 hours; earlier still, we find the day still less and less, but we shall not halt at any intermediate stage; let us at once project our view back to the earliest and the most interesting epoch in the history of the earth-moon system. At the very remote epoch to

XUM

which I refer, the day was very much shorter than our present day. It was, indeed, only a small fraction of 24 hours. We cannot be sure of the precise number of hours in the day at that time; it seems to have been more than two hours and less than five hours. For simplicity, we may speak of the length of the ancient day as about three hours, but it must be carefully remembered that this estimate is to be regarded merely as provisional, though perhaps approximately correct.

It had long been known that the tides were increasing the length of the day, but the very remarkable researches now to be described have only been made quite recently. They are the work of Mr. G. H. Darwin, Fellow of Trinity College, Cambridge. Mr. Darwin's labours are contained in a series of memoirs of a very abstruse nature, and we here propose to give a general sketch of the principal results to which he has been conducted, so far as the earth-moon system is concerned. We shall endeavour as clearly as possible to indicate what portions of the theory are to be regarded as absolutely established and what portions are still more or less speculative. We have halted in our retrospect at a day of 3 hours. Why is it that we cannot look much earlier—to a day of one hour, for instance? There is a very good reason why we cannot do so. In those exceedingly early times, our earth was not the hard rigid body with which we are familiar. It was in those days so heated as to be quite soft, even if not actually molten. A body like our earth in a molten state will not remain in a spherical form when it is rotating on its axis. It will bulge out at the equator; it will become flattened at the poles. The greater the velocity, the greater will be the protuberance at the equator. If, indeed, a certain critical velocity be attained, it will be impossible for the body to hold together; the centrifugal force would be too great, and a rupture of the body must ensue. It is not practicable to calculate what that critical velocity may be. The critical velocity depends upon circumstances not within our present knowledge, but it can be shown that the velocity does not differ, perhaps, very much from a rotation once every 3 hours. We thus see that a rotation of this amount is about the greatest that our earth could ever have had in the present order of things. What occurred prior to this is not to be discussed at present.

We now turn our attention to the moon, which, in consequence of the tides, must be describing an orbit of gradually-increasing dimensions. It may be well to remind the reader that the orbit of the moon is at any moment a nearly circular ellipse, and that this ellipse is subject to many disturbing influences well known to astronomers. But these disturbances are all periodic. They increase and they decrease. They may, in the course of ages, be entirely overlooked in comparison with the tidal changes, which constantly act in the same direction. In very ancient days, the moon must, therefore, have been nearer to the earth than it is at present. The further we look back, the nearer must the moon be. There is no reason why we should not look back to an excessively remote time, when the moon was as near as possible to the earth. The most extreme case would arise when the moon was so near the earth that the two bodies were almost touching, and we are bound to believe that, at some inconceivably remote epoch, this did actually happen. It is easy to calculate what must then have been the length of the month, or the time which the moon occupied in completing one revolution around the earth. Kepler's law shows that when the moon completed one revolution around the earth in three hours, the two bodies must have been very close together. There was thus a certain very critical epoch in the earth-moon history. At that time the earth and the moon were close together; the earth was spinning round on its axis in three hours, and the moon was revolving around the earth in the same time. The three hours is, as already remarked, open to some uncertainty; but there can be no doubt that at this critical epoch the earth was turning round in the same time as the moon, whether that time be three hours or some other amount of about the same magnitude. At this interesting epoch the earth kept the same face directed towards the moon, and the moon kept the same face towards the earth. In fact, the two bodies revolved just as if they were bound to each other by invisible bands.

MICROSCOPIC VISION AND MINUTE LIFE.

By Henry J. Slack, F.G.S., F.R.M.S.

In the early days of the microscope, wonderful reports were made of its revelations, and in 1745 Baker complained that "some people made false pretences and ridiculous boasts of seeing by their glasses the atoms of Epicurus, the subtle matter of Des Cartes, the effluvium of bodies, the emanations of stars, and such-like impossibilities."

One doctor declared that he had seen the effluvium of magnets as a mist. Probably he mistook a misty view for a view of a mist. As the instrument improved, and more knowledge was gained, the sham wonders ceased to appear, and although little was known of the molecular construction of matter, it was no longer imagined that its minutest or ultimate particles would be seen with the powers employed. We are not yet in a position to say exactly where the limits of vision must necessarily end. Dr. Pigott reduces the image of a spider's web to less than one-millionth of an inch in diameter, and then shows it by remagnification. His process is like diminishing an object by looking at it through an inverted telescope, and magnifying the small image thus obtained. After proving that "spider lines, miniatured down to the fourteenth part of the hundred-thousandth of an inch, could be made visible to ordinarily good eyesight, under proper* microscopical manipulation," he sought for actual objects comparable in minuteness with these optical images, and succeeded in showing them. One plan he adopted was to smash, with a watch-spring, very small globules of mercury in a minute drop of petroleum, inserted under a thin cover on a slide. Many of these mercurial particles thus obtained were exceedingly minute, some round, and others irregular. Upon some of the irregulars he found minute black points, visible with a power of 1,000 diameter, and comparing them with the thinnest spider line, he found one, in particular, less than one millionth of an inch in diameter.

The visibility of extremely minute objects depends much upon their optical properties, and how they are situated in reference to neighbouring bodies. In his remarkable investigation into the life-history of certain small objects, called monads, Mr. Dallinger employed a magnification of five thousand linear, and could not, with this great power, see the minutest germs capable of development into active organisms. In the course of the spontaneous germination controversy, the extreme minuteness of these germs was not dreamt of by advocates of that theory, and Pouchet thought the "panspermists," as he called those who adhered to the doctrine of *omne vivum ex ovo* in the sense of regard-

* "Proc. Roy. Soc.," p. 208. 1880.

ing every living thing as the offspring of a previous living thing, quite overthrown by considerations of the density air must have if it were as full of germs as they supposed. Pasteur, Dallinger, and Tyndall have completely destroyed this argument, and shown that germs in vast numbers can exist floating in the air without any appreciable addition to its weight, or obvious diminution of its transparency, though, as we shall see, that is easily affected. Germs of various kinds are most numerous in the air of towns and inhabited rural districts. They become fewer as mountainous heights are ascended, and in well-selected situations disappear entirely. All ordinary air will cause life to appear in appropriate fluids that have been previously rendered sterile; but if a bottle containing such a fluid is opened with due precautions on a mountain peak, and then hermetically sealed, no life is developed. In such experiments Pasteur used glass vessels with their necks drawn out, and sealed up by melting them in a spirit lamp, or with a blow-pipe. To ensure against accidentally introducing any germs he might have carried with him up the mountain, he broke the tips of the vessels' necks with pliers made hot in a spirit lamp, and, after air had been admitted, instantly closed them by remelting. He thus found pure air to be free from any life-producing particles. Tyndal's experiments were made by imitating the well-known motes in a sunbeam. He found that a beam of electric light gave evidence of amazingly minute particles floating in the air, and that when this effect entirely ceased, the air contained no germs. We can seldom form an accurate idea of the real size of the minutest objects we can just discern with the microscope. They often look a good deal bigger than they are, through the optical defects of the instrument and the eye, though Dr. Pigott found that when all the conditions can be rendered favourable, the error is very small. The smallest floating particles lit up by Tyndall's artificial sunbeam are too minute for individual recognition by the microscope. It is only when they are numerous enough to form a delicate cloud that their presence can be made manifest. Objects while floating in the air could not possibly be seen with high powers. It is necessary to collect them, and keep them either still, or only moving in some fluid with moderate velocity. If they are very nearly of the same refractive power and colour as the fluid in which they are immersed, they can only with great difficulty be distinguished at all. Naturalists and physiologists can seldom arrange all the circumstances in the way most favourable for attaining to the extreme limits of vision, and their researches are usually made within much narrower limits. If, however, the utmost possible power of the microscope could always be employed, it would not bring us near the probable limits of organic life. The minute organisms capable of inducing changes analogous to the fermentation caused by yeast have received great attention of late years, and several important diseases are distinctly traced to them. Béchamp estimated that eight thousand millions of germs of one micro-ferment only occupied one cubic 25th of an inch. Not one of these minute bodies could develope except by carrying on complicated processes of a chemical nature, involving very active movements of its atoms and molecules.

The mathematicians have made calculations founded upon the pressure exerted by gases, and other considerations, which show that a particle of the sort of matter, such as albumen and protoplasm, chiefly concerned in life processes, contains in a space of one cubic thousandth of an inch more molecules than any one could possibly form any conception of. Sorby, taking a probable mean of such calculations, supposes one cubic thousandth of an inch of water to contain 3,700,000,000,000,000 molecules. A sheet of ordinary note paper is about one-hundredth of an inch thick. One-tenth of this would, of course, be one-thousandth of an inch, and a little square box of that size each way would hold the amazing number of water molecules mentioned. Perhaps a few thousands of such molecules may suffice for some manifestation of life, but even if many millions should be requisite for the structure of the humblest and simplest germ, we could never expect to see the actual beginnings of life.

When one million is spoken of, few persons form any definite conception of the quantity meant, and billions, trillions, quadrillions, &c., convey no graduated conceptions to anybody except in the roughest way. Mr. Samuel Butler, in his work on "Unconscious Memory," states that "a man counting as hard as he can repeat numbers one after another, and never counting more than one hundred, so that he shall have no long words to repeat, may, perhaps, count ten thousand, or a hundred a hundred times over, in an hour. At this rate, counting night and day, and allowing no time for rest or refreshment, he would count one million in four days and four hours, or say in four days only. To count a million a million times over he would require four millions of days, and roughly ten thousand years. For five hundred millions of millions he must have the utterly unimaginable period of five million years." And yet in how small a space the matter around us contains molecules to this inconceivable extent! The things unseen far surpass in number, as in minuteness, the things seen.

THE LAST TRANSIT OF VENUS.*

By the Editor.

MY friend, our F.R.A.S., used to say the transit of Venus was, with me, like King Charles's head with Mr. Dick. The resemblance was certainly striking; Mr. Dick was always trying to keep King Charles's head out of the Memorial, and constantly failed; I spared no efforts to bring the transit of Venus before the public, and always succeeded. Be this as it may, it is certain that, except in the case of an event like the transit, which was bound to come off at a particular time, matters of scientific discussion are generally none the worse for waiting. Certainly, now that the transit is over, and no good can arise from any discussion of the best ways of observing it, I should have thought myself very unlikely to go again over the well-worn ground, or to recall the circumstances of a long past controversy: is not the story told in the Encyclopædia Britannica, in the American Cyclopædia, and in the Monthly Notices of the Astronomical Society?

But I must confess the introduction of the treatise before us—a treatise giving ample evidence of the zeal and energy with which Sir George Airy could do any work to which he gave his mind—has somewhat changed my views as to the desirability of silence. There is not a word which is absolutely untrue in these pages; but there is a quiet *suggestio falsi*, a calm and complete *suppressio veri*, which I cannot but consider *par trop fort*. Let me briefly run through the facts of the case.

In 1857, Sir George Airy made a communication to the Astronomical Society, in which a comparison was made between the transits of 1874 and 1882, with regard to the suitability of the two chief methods for observing these

* Account of "Observations of the Transit of Venus," 1874, December 8. Printed for the Government Stationery Office, under the authority of the Lords Commissioners of the Treasury, 1881.

phenomena. In 1864, May 5, he addressed another communication to the same body, advocating—*for the observation of the transit of* 1882 *only*—a *reconnaissance* of Antarctic stations. In 1868 he made yet another communication, advocating the same views, and an active discussion followed, in which the Hydrographer of the Admiralty, Captain (now Sir G.) Richards took part, and in which it was generally agreed by the naval officials present that Antarctic stations could, and should, be occupied, as suggested, for observing the later transit.

It chanced to be my duty at that time to write the reports of astronomical progress for the "Quarterly Journal of Science," and for the "Popular Science Review;" and having the belief that such reports should not be limited to mere quotations from those who are assumed to be authorities, but should involve a little independent inquiry, I began the investigation of the problem which I then supposed that Sir G. Airy had most fully and satisfactorily dealt with. But I found his investigation to be incorrect. A certain assumption at the beginning, which had every appearance at first sight of being right, turned out on closer inquiry to be altogether wrong. To give an idea how wrong it was, I need only point out that the method deemed most suitable for the earlier transit turned out to be the only method available (of the two in question) for the second, while the method regarded as only available for the second turned out to be far and away the best for the first transit.

After calling Sir G. Airy's attention to this matter in a courteous letter (supposing, of course, that when once he had noticed the error he would set it right himself), and receiving from him (as on several other occasions) a reply more curt than courteous, I set to work to complete my investigation for publication, and I eventually communicated it to the Astronomical Society. Its accuracy was never questioned. Sir George Airy admitted in a letter (by no means intended to say pleasant things) that it was the most complete and accurate discussion of the transits published up to that time. It was only open to one exception; it was not official: and because it was in no sense my duty to make this investigation (in other words, because I was not paid for doing the work), some (chiefly minor officials) fondly imagined that I had no right to make it.*

Now, in this complete investigation of the matter, I was able to demonstrate two points of great importance—first, the utter inadequacy of the arrangements suggested for the observation of the transit of 1874; and secondly, the utter uselessness of the proposed Antarctic expeditions for observing the transit of 1882. Thirdly—but scarcely of less importance—I noted a region in British India, including several of the best stations for observing the transit, and heretofore overlooked (because of the singularly unsatisfactory method of mapping the observing districts which Sir G. Airy had unluckily adopted). To these important matters I called attention more publicly than by papers read before the Astronomical Society, viz., in an article which appeared in the *Spectator* of February, 1873. My views were stoutly and skilfully supported by Sir Edmund Beckett, in a paper which appeared in the *Times* of Feb. 13, 1873.

Note that in these papers it was shown (1) that a number of northern stations where the whole transit of 1874 could be observed should be occupied, for which kind of observation *no provision at all had so far been made;* (2) that no stations need be occupied for observing the whole transit of 1882, and *especially that the dangerous Antarctic stations could not possibly be occupied with advantage then* (but it was noticed that if they can be occupied at all, they should be occupied in 1874, to supplement stations already provided for in the south, where, *as it happened*, though whole-transit observations had not been intended, such observations could be made); and (3) that the North-Indian region mentioned above should be occupied.

Government, of course, followed the customary official course,—inviting the officials whose judgment was oppugned to say whether they were mistaken. Equally of course, those officials said they had made no mistake, implying even, by their tone, that officials never do, or can, make mistakes.

Sir G. Airy tried the same line with the Astronomical Society. He pooh-poohed the notion that Siberian and north Chinese stations could possibly be occupied—and a fortnight later news came that American, Russian, and German astronomers were to occupy these very regions. He ridiculed the North Indian region, which he had overlooked—and very soon after he had to provide for extra stations in that very region. But he specially ridiculed the suggested Antarctic expeditions (one of the islands—St. Paul's—which I had recommended, was eventually occupied by the French, and good work done there), as if I had ever had any reason but his own advocacy of such stations (ridiculous advocacy, he now asserted) for believing that they could be occupied. And of course, the very officials who, when he had wanted the stations for 1882, had urged no objections, now swallowed all they had before said, and—greatly daring—said the very opposite.

Just here, *where I had gone wrong in following him and believing in official utterances, was the one point where what I had advocated was not carried out in every detail; and just this point is all that Sir G. Airy chooses to notice in the introduction to the volume before us.* He describes my paper in the *Spectator*, and Sir E. Beckett's, in the *Times* (three columns), urging most important changes, which had eventually to be adopted, as papers "strongly urging the adoption of Enderby Land (which, after careful consideration, I had rejected) for a southern station." As a matter of fact, Sir G. Airy never had rejected Enderby Land for 1874; he had never thought of it; he had urged Antarctic stations for 1882, and had only given them up after I had shown that such stations, useful enough astronomically in 1874, would be of no adequate value in 1882. He gave up Antarctic stations simply because, if they had been occupied at all, they must (after what I had shown) have been occupied for a purpose which he had himself overlooked. *I have not a shadow of a doubt, after carefully studying what was said by Airy, Richards, Ommanney, Davis, and Stone, on Dec.* 11, 1868, *that but for my demonstration of the astronomical uselessness of Antarctic stations in* 1882, *we should have had, before now,*

* Strangely erroneous ideas are very common about official position. A man is appointed to an important office in order that he may do certain work, for which he is to be more or less handsomely remunerated, out of money provided by the tax-payers. If this officer is at the head of a department, he has, besides his salary, authority over all other officials in that department. But many seem to imagine that this authority extends to persons outside official circles—an idea preposterous on the face of it; for such persons are in reality among the employers of such officials, paying them to do certain work, and having a right (if they chance to have the necessary knowledge) to inquire how the work is being done. The trouble is, that while so many have the right, so few have the knowledge. Those who have it, if they possess also the time to do the work, have something more than the right—it is their duty to make the inquiry. Who else is going to do it? If—which is altogether unlikely—our present able and active Astronomer Royal were grossly to neglect all the duties of his office, who is there above him in office who could indicate his shortcomings? and who is there below him in office who would venture to. Somebody outside of office *must* do the work.

to pay for expeditions to Possession Island and other places in the dangerous Antarctic seas, and to provide for stations to be occupied there during the transit of next December. It was this pet plan of his which Sir George Airy was really giving up, when he explained to the Government that Antarctic stations were "geographically unsuitable," and so forth.

It is a favourite argument of the junior officials who tried to earn approval from their chief by attacks on extra-officials like Sir E. Beckett and myself, that Sir G. Airy never did yield as to the stations for observing the whole transit. It is not quite true. He yielded in the case of the Indian stations, which astronomers of other countries were not likely to occupy. I think too highly of his real zeal for science to suppose he would not have yielded in the case of Siberian and North Chinese stations, if America, Russia, Germany, and France, by occupying the stations he had overlooked and later stigmatised as useless, had not saved him from the concession.

Like all that Sir George Airy has ever done in this way, the record of the observations made by the various parties is exceedingly well arranged. His labours here would have served to very much more than retrieve what, after all, was in its inception but an unlucky mistake (which might easily have been corrected, and the world none the wiser, if he had not been so needlessly impatient of extra-official suggestions). Sir George Airy shows himself in the body of this work what he has shown himself during his whole tenure of office, the most energetic and laborious of our Astronomers Royal. If he had left out all reference to the discussion of 1869-1873, or if, referring to it, he had told the whole story, this would have been all I need have said; but I have not thought it just, either to myself or those who by their aid ensured the adoption of proper measures for observing the transit of 1874, to allow an imperfect and entirely misleading account of the matter to remain uncorrected—though I know very well that for one who might be misled by Sir G. Airy's inexact account, thousands (including himself) know how the matter really stands. It seems almost incredible that the writer of this account should be the same man who so frankly and nobly acknowledged his error in the Adams-Leverrier controversy.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Second Notice.

THOSE who can look beneath the surface will see that during the past week some considerable progress has been made towards completing the exhibits. To a cursory examiner, however, but little improvement can be apparent. A few Jablochkoff lamps (*Compagnie Générale d'Electricité*) are lighted, but present a poor appearance when compared with the other systems. The four Cromptons which light the space in front of the stage are also far from satisfactory. We may hope to see something better in the telephone section during the next week or so.

We will now give a description of

The Brush System

of electric lighting. In the machine-room, as mentioned last week, are a number of generating machines, which, when all in work, will be driven by five steam-engines, two of them of 20-horse-power, two of 30-horse-power, and one of 25-horse-power. The latter is the only one at present fitted up. It is working up to about 45-horse-power, and drives three of the generating machines, two of which maintain between them thirty-two arc lights, and the other about fifty incandescent lamps. The third machine is, however, capable of supplying the current for as many as 150, or perhaps 200 lamps.

The lamps are very simple in construction, requiring no adjustment after being once put in order. Fig. 1 is one

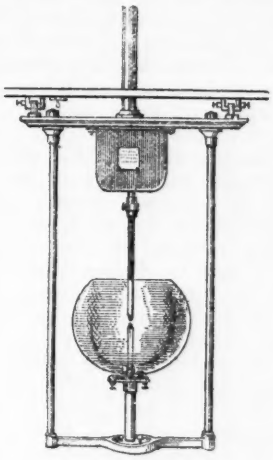


Fig. 1.

of the ordinary forms which will burn uninterruptedly for eight hours. The carbons are made in 12-in. lengths, and when the lamps are required for more than eight hours together, they are furnished with a double or even treble supply of carbons, the current passing from one pair to another automatically. Fig. 2 is a somewhat rough

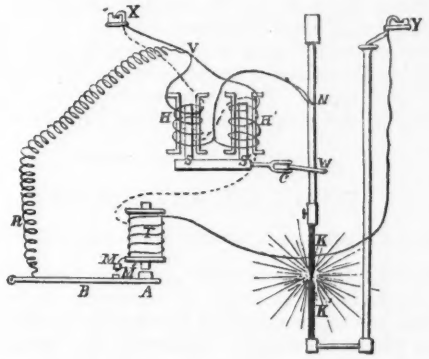


Fig. 2.

diagram illustrating the principles and action of the lamp. The current enters at *X*, and at *V* splits into two sections, each of which passes through a small coil of thick wire *H H'*, and on leaving the coils the currents combine again and pass to the upper carbon rod; thence through the arc to the lower rod, and out at *Y*. From *X* there also passes a thin wire which is coiled many times round *H H'* outside the thick wire. These thin coils are wound *in series*, and on both bobbins they are *in opposition* to the thicker coils. The thin wire then passes several times round another bobbin *T*, carrying a small coil of thick wire wound in *the same* direction. The circuit is completed by a wire to *Y*.

The lever, *A B*, is in metallic connection with the positive pole, and carries a small button, *M'*, over which, but not

normally touching it, is another button, *M*, in connection with the thick wire attached to the wire leading to *Y*. *W* is a washer or clutch, consisting of a small circular disc of brass fitting loosely on to the carbon-holder, so that, if slightly tilted, it takes a grip on the holder on both sides, and prevents the carbon-rod falling. When the current enters, the rods being in contact, a circuit is formed. The current divides between the thick and thin wires on *HH'*, only 1 per cent., however, going through the thin wire, its resistance being 450 ohms, while that of the thick wire is only 4·5 ohms. The current going through the thick wire draws up the soft iron plungers, *SS'*, and with them the washer, *W*, which pulls the carbon-holder up a short distance, and so creates an arc. When the arc gets too long, the resistance of the thick wire circuit is considerably increased, while that of the thin wire circuit is relatively decreased. Consequently, the upward tendency imparted by the thick wire is counteracted by the greater amount of current passing through the thin coils, and the holder falls until it is again restrained by the clutch. Should it so happen that the carbon becomes broken, or that by any means an arc cannot be formed, the whole of the current passes through the thin circuit, and in passing through *T* is sufficiently powerful to attract *A*, thereby putting *M* and *M'* in contact. The current then flows from *X* along lever *BA* through *M'* to *M*, through the small, thick wire, and away to *Y*, thus cutting the carbon-rod completely out of the circuit. This "cut out" allows the lamps to be joined up in one circuit, without the risk of all of them going out in the event of one being faulty, a feature belonging only to this and one other form of lamp.

Preparations for further lighting are being made on a magnificent scale; a 30-horse-power engine is to drive one of the largest-sized machines, which, it is said, will maintain one light of 150,000 candle-power, and from another machine a light of 50,000 candle-power is to be directed towards the Houses of Parliament.

A magnificent display is being rapidly prepared in the Alhambra Court, which promises to outdo everything else in the Exhibition. We must, however, defer any further remarks on this company's exhibit until a future occasion.

POPULAR ASTRONOMY

By the Chief of a Great National Observatory.*

SEVERAL correspondents, in the course of their inquiries as to books on astronomy, have asked us whether we can recommend the treatise written by Professor Simon Newcomb. Although the book has been more than two years before the public in this country, as well as in America, we deem it well, in response to these queries, to give an account of it, as manifestly many in England are unaware of its merits.

We do not, as a rule, much admire the way in which *soi-disants* professional astronomers treat the wonders of the heavens and the grand problems presented by the movements of the celestial orbs. Too often they discuss these as a mere land surveyor might discuss the teachings of the earth's crust. Methods and instruments of observation are of much more interest in their eyes than the lessons to be learned from observations after these have been carefully made. They seem, at times, even disposed to be angry, as Flamsteed was with Newton, when the raw materials which they have gathered together are worked into the manufactured article—Knowledge.

It is a pleasant disappointment, then, to find that Professor Newcomb, the chief official astronomer of the United States, does not take so limited a view of popular astronomy. In his "Popular Astronomy," he deals more or less fully with every part of the science which could possibly be of interest to the general reader. He gives a full, though condensed, view of the history, principles, methods, and results of astronomical research. The historic and philosophic sides of his subject are treated with more fulness than is usual in works of this kind. The purely technical side has been somewhat condensed, but is dealt with as fully as is necessary.

Having briefly treated in the first chapter of the apparent motions of the heavenly bodies, and described the Ptolemaic system, Professor Newcomb proceeds in the second and third to describe the work of Copernicus, Kepler, and Newton, in establishing, step by step, the true system of the universe. These three chapters form the first part of the work. The second part deals with practical astronomy. The picture of Bianchini's monstrous tube, as mounted in the grounds of the Barberini Palace at Rome in the seventeenth century, gives a good idea of the difficulties with which the earlier observers had to contend. A telescope that a child could handle would, in our day, give better views of the heavenly bodies than this unwieldy instrument, the moving of which required the efforts of several men. After a description of the modern achromatic telescopes and reflectors, there follows a section on the magnifying powers of telescopes, which we would recommend to the especial study, not only of observers, but of those who have occasion to discuss observations. In particular, this section will serve to correct the common error that large telescopes increase the apparent brightness of objects which present a visible surface, as distinguished from objects like the stars, which even under the highest telescopic powers appear as mere points. We could have wished, however, that this error had not been corrected merely by an *ex cathedrâ* statement, but that the reasoning establishing the true relations between magnifying and illuminating power had been given in full. The second part includes also an account of the application of the telescope to celestial measurements, a most interesting and valuable chapter on measuring the distances of the heavenly bodies, and chapters on the motion of light and on the spectroscope.

The third and fourth parts of the work are devoted to descriptive astronomy. In the former, after a chapter on the general structure of the solar system, we have a long and very interesting chapter on the sun, and chapters on the inner group of planets (including the earth and moon), on the outer group or family of giant planets, and on comets and meteors. The fourth part deals with the stars—first, as they are seen with the naked eye and with the telescope; secondly, as they are probably arranged in space; and, thirdly, as each has probably been formed. The third chapter discusses questions of extreme interest, such as the sources of our sun's heat, the secular cooling of the earth, the plurality of worlds, and the nebular hypothesis. Lastly, we have lists of the principal telescopes of the world, and catalogues of double stars, nebulæ, star clusters, and comets; the usual tables of elements (but greatly improved in many respects), a useful glossary, and a set of star maps.

One general characteristic of these various chapters may be broadly indicated. There is scarcely one statement, properly so called, in the whole work which is not strictly accurate, or rather, which does not present with strict

* "Popular Astronomy." By Simon Newcomb, LL.D., United States Naval Observatory. (London: Macmillan & Co.)

XUM

accuracy the present views of astronomers in relation to the subject matter. This remark is not intended to include expressions of opinion on the one hand, or, on the other, such statements as are subsidiary to explanations or illustrations, but only definite statements respecting astronomical matters. With regard to all, or very nearly all of these, the reader may trust our author with perfect confidence. In this sense the work deserves the high praise accorded to it, of being thoroughly trustworthy.

The arrangement of the work throughout is admirable, and the treatment of individual subjects is at once lucid and attractive. Here and there the style is rather commonplace, but it is never confused. Every sentence has one meaning and one only. Moreover, the meaning of every sentence lies on the surface. In these respects, too, many who write on scientific subjects have failed to consider the requirements of the general reader. To the mental effort which the study of such subjects requires, they add the effort required to determine the meaning of ambiguous phrases. Moreover, Professor Newcomb commonly avoids the mistake of using technical terms where ordinary terms would serve equally well.

The treatment of the subject is simple in another sense, perhaps not quite so satisfactory. Professor Newcomb leaves many matters unexplained which are commonly explained with more or less fulness in treatises on general astronomy. For instance, in nearly all such treatises we find some explanation of these disturbances of Uranus by Neptune which led to the discovery of the latter planet. The explanation is usually incorrect, but that is a detail. It always requires more or less mental effort either to follow the explanation, or to discover that the explanation is, as usual, all wrong. No such effort is required in reading what Professor Newcomb says respecting the discovery of Neptune, simply because he contents himself with a mere statement of facts. Possibly this was the wisest course in the case of a treatise intended for general reading. It may be doubted whether the true explanation given by Sir J. Herschel, for instance, has been followed by a tenth of his readers, or whether one-tenth of the readers of Lardner's, Chambers's, and other such works, detect the error underlying incorrect explanations of this subject. It is necessary, however, to remind the reader of the work before us that the ease with which he follows the author here and elsewhere is due to the circumstance that difficulties are avoided—they are not overcome. This is even the case with Professor Newcomb's treatment of the subject of the sun's distance, which few have mastered more thoroughly than he has. His statements respecting the various methods available for determining the distance of the sun are thoroughly trustworthy, and his opinion respecting the result to which observations point may be accepted as the one which accords best with the evidence. But the matter is not explained. We are able, indeed, without making too long an extract, to quote all that he says in the way of explanation, which is simply this :—

"In consequence of the parallax of Venus, two observers at distant points of the earth's surface, watching her course over the solar disc, will see her describe a slightly different path, as shown in Fig. 50. It is by the distance between these paths that the parallax has hitherto been determined."

The general nature of parallax has been already explained, and the distinction between Halley's and Delisle's methods for determining this distance is briefly indicated further on. But the principle underlying both those methods, as well as the photographic and heliometric methods, is explained no further than in the sentence quoted above. It is not too much to say that it is not explained at all.

An important and, in our opinion, a valuable feature of this work is the discussion of ideas more or less speculative with regard to the heavenly bodies. There are some astronomers who object strongly to the introduction of ideas of this sort into treatises on astronomy. But the objection seems unwise, not to say peevish. The chief charm of the study of astronomy lies in reality in the mystery which enshrouds the orbs of space. What we know respecting these bodies is little; the unknown is infinite. Now, unquestionably, mere idle speculations, not even suggested by observation, are profitless. But speculations based on the results of observation and physical research are not only interesting, but fruitful. In such speculations have originated nearly all the hypotheses from among which the established theories of the science have been evolved. It is noteworthy that the greatest astronomers have indulged freely in speculations respecting the unknown. Copernicus, Galileo, Kepler, Newton, the Herschels, and a host of those to whom astronomy owes the noblest of its triumphs, have discussed speculations and hypotheses, of which some have been forgotten, others are remembered only because of the theories which they suggested, while some few have become themselves the theories of the science. It may, indeed, be truly said that no astronomer who has been unwilling to allow his thoughts to pass beyond the boundaries of the known has ever made great discoveries. We are glad to see that Professor Newcomb, while he has shown himself an exact and careful observer, and while, in certain departments of mathematical research, he has held his own with the greatest mathematicians of our time, is ready to consider subjects which belong as yet to the region of speculation. He presents, for example, the various theories respecting the sun's condition which have been advanced by Secchi, Faye, Langley, and Young, though three of these are certainly, and all four possibly, erroneous. He adopts, with apparent approval, several opinions respecting the condition of the larger planets which have been advocated in recent years in this country. He accepts the conclusions of the Bonds, Peirce, and Clerk Maxwell respecting the condition of Saturn's rings. And speaking generally, he presents astronomy, not as a subject respecting which certain facts and statements have to be committed to memory, but as a living science, full of promise, though also full of mystery, presenting a grand array of achievements in the past, but offering also a vast number of noble problems for attack, and possibly for solution, in the future.

The present work, it will be inferred, is a very valuable contribution to astronomical literature. It is especially characterised by originality of tone and treatment throughout. It is remarkable also for the care with which details have been attended to, quotations verified, tables corrected, and so forth—matters respecting which scientific writers of eminence are not always so careful as they might be.

(To be continued.)

Intelligence in Animals.—J. H. sends the following:—"An amusing account of a pet baboon, in a letter from a friend at Zanzibar is communicated to *Nature* this week by Miss Julia Wedgwood. An interesting statement (in relation to the contention that laughter is one of the distinguishing attributes of man) is, that 'Judy,' the baboon in question, used, when she romped with her mistress, 'to open her mouth, show all her white teeth, and regularly laugh like a child, especially when she was tickled.' She never laughed at a joke, and nothing made her so savage as being laughed at."—[Darwin gives similar instances in his work on the "Expression of the Emotions."—Ed.]

XUM

Reviews.

SIR EDMUND BECKETT ON THE LAWS OF NATURE *

SIR EDMUND BECKETT'S writings are always delightful, whatever his subject, and whether one agrees with him or not. His subject in the book before us (which has been some time before the public, but the new edition has not), is recondite, and Sir Edmund's views are strongly opposed to those which are generally held by men of science in these days. But from the first page to the last, the book is attractive, if only through the clearness of the reasoning and the strength of the style. There is not a writer living who has a style more markedly his own than Sir Edmund Beckett, and though Carlyle, among writers who have passed away, had a style more marked than Sir Edmund's, the peculiarities of the author of "Sartor Resartus" were assumed, whereas those of Sir E. Beckett are the result of true mental idiosyncrasies.

In the present work, Sir E. Beckett has very plainly—and, on the whole, very fairly—presented the issue between the believers in special creative, and as it were legislative, acts on the one hand, and in evolution pure and simple on the other. He has shown well the insufficiency of evolution as at present understood as regards explanation of the mysteries of the universe, and he has pointed out very definitely the sufficiency of the theory of an omniscient all-powerful Being to account for all the phenomena, including the existence of uniform laws existing so far as can be seen throughout all space and operating during all time. An evolutionist might with equal clearness, we conceive, point out the utterly inconceivable nature of such a Being on the one hand, and the sufficiency of laws of evolution within the range over which scientific research can extend to account for those observed relations which, referred to an absolute beginning, seem only explicable on the hypothesis of special creation. After all, what is science now doing but somewhat extending the range over which uniform law may be *seen* to extend its influence (not the range over which it actually does so)? What an overwhelming thought it would seem to an ephemeron that a giant tree which, during the brief lives of millions of his race, had seemed scarcely to change except in its leaves or blossoms, had been developed to be what it has been during the continuance of generation after generation of his kind! Nothing in the widening of the domain of law which has been seen during the last century can be compared to the tremendous nature of such a revelation to a being who had regarded the pushing of a bud or leaflet as the limit of the operation of natural laws. And yet how little such a revelation compared to the conception that a whole forest had grown, and that the very earth in which it grew contained the remains of past generations of trees. Science is widening somewhat like this our conception of the extension of law. But the man who thinks that this widening of the domain of law means the rejection of a Law-giver; or that, by carrying back the operation of dependent causes a few steps—or even (were that possible) a few millions of steps—we get rid of the necessity of recognising a First Cause, must be strangely-minded indeed. For such a one, the book before us will be useful; to those who view aright the operations of nature, it cannot fail to be interesting.

Two points have occurred to us as open to exception in this book. One is the reference to Tyndall's work on the subject of spontaneous generation, as a "curious retribution" (though Sir E. Beckett frankly admits it is creditable to the honesty of the most eloquent prophet of the doctrine of evolution). If there is one feeling which Tyndall has shown more strongly than another, it is the desire to come at the truth, and surely another description might be found for one of the most interesting of Tyndall's labours in the search for truth. Tyndall has found, indeed, that in a particular direction the beginning of life cannot be found. Darwin has shown that, in his opinion, no researches men can make will take them to the beginning of life, any more than the study of second causes will lead men to the First Cause. All this is in perfect harmony with the views of evolutionists—it is no retribution, but a confirmation of their views. The other point to which we would take exception is the remark thrown in more than once, that natural selection acts "for no cause, so far as we can see." The cause has been repeatedly indicated by evolutionists—this, namely, that those who have not the qualities in question die out; surely the death of those who have not such qualities is a tolerably good reason for the selection of those who possess them. We may note again that our author somewhat too confidently assumes that certain qualities could be of no use till fully developed; that, for instance, until or unless spiders made perfect webs, they might as well make none at all. We know that imperfect cell-making by certain orders of bees is better than utterly bad cell-making or no cell-making at all. Why should it not have been so in the past with spiders? A few lines of web might have been useful—even a single line, however short—in the earlier stages of the struggles for existence through which the Arachnida have passed.

* "Origin of the Laws of Nature." By Sir E. Beckett, Bart., LL.D., F.R.A.S., second edition. (London: Society for Promoting Christian Knowledge, 1880.)

THE FOOD WE EAT.*

This is a useful book, though Dr. Fothergill's instructions are not always so definite as they might be. He tells us roast mutton has a baneful history; but there is nothing in the chapter on flesh meat to suggest that we are too carnivorous; on the contrary, we read, "beef and mutton are the meats allowed by the trainer and prescribed by the physician: and the choice is a wise one, if not made too absolute." If it is suggested in one place that Cain killed Abel in a fit of irascibility brought on by gout—the result of flesh food—it is carefully explained in an editorial note that for this theory there is no authority in the Hebrew record. The rules for the taking of alcohol are sensible. They are these:—It is well to do without alcohol during the day; alcohol may be taken at bed-time, with advantage, by those whose sleep is broken by worry" (yet what poor rest whisky-bred sleep gives); "when a little wine or its alcoholic equivalent enables a person to take a little food when exhausted and digest it, which otherwise could not be taken, it is permissible." Taking "something" early in the day to set one up, is, as our author well says, the best way to destroy working energy, and alcohol is the worst possible resource against trouble. The narcotic dose recommended as at times a useful sleeping draught, is too much for any but confirmed topers—it is "a dose at least twice the amount usually taken in a tumbler." In the chapter on fruit, our author says that the raspberry is scarcely second to the strawberry; *de gustibus non est disputandum*, but many prefer raspberries. It was not "an irreverent American," by the way, but the Rev. Cotton Mather, a devout

* "The Food We Eat." By J. Milner Fothergill, M.D. (Griffith & Farran, London.) Price, 1s.

XUM

Calvinist, who said that "doubtless God could have made a better fruit than the strawberry, but doubtless he never had."

LANDAUER'S BLOWPIPE ANALYSIS.*

THE second edition of this very excellent little book, which has just (last November) appeared at Berlin, should have the effect of causing Freiberg to look to its laurels, or that time-honoured head-quarters of Plattner, Richter, and the blowpipe, will have to give place to Brunswick, where this work is written.

I believe Herr Landauer was the first, and is now one of the few German writers on this subject, to point out that blowpipe analysis properly studied, is a strictly chemical, qualitative process "in the dry way," by which the constituents of minerals, as of any other inorganic substances, may be, if possible, separated; and, if separation is impossible, at all events, exhibited so as to be easily and separately recognised in presence of each other. It is most creditable to Landauer's scientific conscientiousness and literary intrepidity, that he should venture to affirm and reiterate this great truth, in opposition to the teaching of such eminent chemists as the venerable Von Kobell, of Munich; of the "great shade" of Berzelius; and even of the departed Plattner himself. All these eminent writers, and some scarcely less respected than they in America, as Professor Brush, have laid down and taught what may be termed a "mineral course of analysis" as contra-distinguished from a chemical course, in which the reactions of minerals *per se*, and not *in partibus*—as a lawyer would call it—are given. Thus it happens that even our best mineralogies are defaced by such distressing descriptions of the blowpipe analysis of minerals, as "fusible on edge"; "melts to a blebby glass, &c., &c.

In England, I have been trying for the last ten years (a paper of mine on the subject was read before the Royal Society in 1872) to preach the same "dry," analytical doctrine; but, as might be expected, have been simply "pooh-poohed" for my pains here. It really seems, however, from the rapidly-succeeding edition of this book, and its translation into English and Italian, as though Landauer would succeed where I have failed, and teach English as well as German "blowpipers" better (analytical) manners in the future.

Unfortunately, it cannot be said that the Manchester translators of this work have been at all successful in rendering what the author has stated in German, into precisely equivalent English, or indeed, in some places, into anything like what he has said. For instance, at page 10, Landauer says of my aluminium plate reactions: "The best substitute for charcoal is the aluminium plate introduced by Ross";† which plain sentence and note, the Manchester transmutationists have replaced by the following ingenious rendering. "As another kind of substitute for charcoal, aluminium plate may be advantageously employed." Many other instances might be pointed out, if space permitted.

It must be admitted that Landauer, with considerable reference to the American writers Elderhorst and Egleston, to his celebrated countryman Bunsen, and to myself, has contrived a "strictly chemical" system of blowpipe analysis, so far as a persistence in the use of the stupid reagents borax and microcosmic (or, as it is now called, "phosphor") salt, permitted. But such a system with these reagents is simply impossible; and Sorby, Wünder, Rose, and others have long ago shown that it is equally impossible to obtain *definite* pyrological crystallisations from the fusion of oxides, &c., in borax before the blowpipe. The plain fact is, as I pointed out ten years ago in my paper (vol. xx. "Proceedings Royal Society") that boric acid will *separate* the constituents of most minerals and inorganic chemical combinations before the blowpipe; whilst borax, or any other reagent, will not. Landauer's book is divided into four chapters and a reprint of Plattner's well-known blowpipe tables. In the first chapter (of fifteen pages) a description of apparatus and reagents is given. In the second (forty-six pages), an excellent account of the necessary operations, including my "Aluminium Plate Reactions" (ten pages). An "Appendix" to this chapter is devoted entirely (eighteen pages) to "Bunsen's flame-reactions," the connection of which, by the way, with the blowpipe, it is difficult to perceive, but it is placed in the book and "table of contents" as the third chapter, though called an "appendix." The so-called third is headed—after the arrangement in Scheerer's and Blanford's little book—"Special Examination for certain Combined Substances," whereby the reader is left to infer that all the other examinations, in many cases the best and most careful, are *not* "special." The fourth chapter describes the author's and Egleston's "Systematische Gänge" (very good Scotch as well as German) or *curricula*; which are both excellent in their way.

At page 3 is given a beautiful woodcut, with a remark that "it is to be specially recommended," of what is called "Rab's Standlöthrohr" (Rab's stand blowpipe), which is not figured in the Manchester translation (1879), because it was not invented by me till 1880, in September of which year a description, with drawings, appeared in the *English Mechanic*. It is, in fact, a mouth-blowpipe with elastic air-reservoir and valve attached, precisely the same as that recently described by me in KNOWLEDGE, vol. i., page 137; only the uncomfortable addition of a huge metallic dish-cover kind of protection to the indiarubber balloon is made, by which all portability and simplicity are ingeniously destroyed. This seems to me too great a sacrifice to make for the sake of elastic bags, which ought not to cost more than a farthing each. I feel sure that if M. Landauer sees this notice, he will at once repudiate all knowledge of my invention when he inserted Rab's drawing and description in his book, which I can cordially recommend to your readers as the best extant on the old system of blowpipe analysis.

* "Die Löthrohranalyse, Anleitung zu qualitativen chemischen Untersuchungen auf trockenem Wege, bearbeitet von J. Landauer. Zweite vermehrte Auflage." (Berlin: Verlag von Julius Springer, 1881. London agents: Trübner & Co.)

† Ross, "Pyrology, or Fire Chemistry." London: 1875.

SCIENCE FOR ALL.

MESSRS. CASSELL, PETTER, & GALPIN call attention to a misprint in the footnote to the review of the last volume of their "Science for All." The price of the volume is not 5s., but 9s. We note that no price is mentioned, but thought we remembered seeing the work advertised for the price named; also, the book sent us was, it appears, the fourth, not the fifth, volume. We took it for granted the latest had been sent. Our remarks apply to Volume IV., not to Volume V. The volumes are unnumbered. Mr. Denning writes (unfortunately at much too great length for insertion), noting that many of the highest authorities—Sir J. Herschel, Webb, &c.—accept the eccentricity of the ring-system as an established fact. That the rings have at times been eccentrically situated is well known; at other times there has been no recognisable eccentricity. Mr. Denning seems to think that saying the Saturnian rings "will be seen at night as a vivid semicircle of light" needed no correction, although the rings are absolutely invisible from large portions of them, not visible as a vivid semicircle at any time from any part of Saturn except the equator, and not visible from the northern hemisphere during one half of the Saturnian year, or from the southern hemisphere during the other half. We venture to say that every reader would suppose Mr. Denning's statement to mean that if you were placed anywhere on Saturn, you would at night see the rings as a vivid semicircle of light. We submit that the statement is as incorrect as the following would be:—On the earth, the sun is visible for twelve hours out of the twenty-four. We are sorry if Mr. Denning objects to being set right in a matter about which there are no two opinions among persons competent to form an opinion at all—that is, in this case, among mathematicians (for the question is purely mathematical).

NIGHT MINIMA OF ALGOL. 1882.

THE following list of night minima of Algol, by the eminent observer of Variable Stars, Mr. Jos. Baxendell, will be much valued by many readers.—ED.

GREENWICH MEAN TIME.

			H.	M.				H.	M.
Feb.	18	....	13	22	Sept.	4	.....	9	35
,,	21	.....	10	10	,,	21	.....	14	29
,,	24	....	7	0	,,	24	.....	11	17
March	10	.....	15	4	,,	27	.....	8	6
,,	13	.....	11	53	Oct.	11	.....	16	11
,,	16	.....	8	42	,,	14	.....	13	0
April	2	.....	13	35	,,	17	.....	9	49
,,	5	.....	10	24	Nov.	3	.....	14	42
,,	8	.....	7	13	,,	6	.....	11	31
,,	22	.....	15	7	,,	9	.....	8	20
,,	25	.....	12	6	,,	23	.....	16	24
,,	28	.....	8	55	,,	26	.....	13	13
July	0	.....	10	51	,,	29	.....	10	2
,,	20	.....	12	33	Dec.	2	.....	6	51
Aug.	9	.....	14	15	,,	13	.....	18	7
,,	12	.....	11	4	,,	16	.....	14	55
,,	29	.....	15	56	,,	19	.....	11	44
Sept.	1	.....	12	46	,,	22	.....	8	33

JOS. BAXENDELL.

XUM

THE MENACING COMET.

By the Editor.*

"SUPPOSING Mr. Proctor's facts to be correctly stated," says the *Spectator*, "there does seem a remarkably good chance that in 1897 the sun may suddenly break out into the same kind of intensity of heat and light which caused the conflagration in the star in the Northern Crown in 1866, when for a day or two the heat and light emitted by it became suddenly many hundreds of times greater than they were before." The *Spectator* is exercised by the inquiry whether the world's belief in science is quite so genuine as it seems, seeing that "Mr. Proctor's warning has not yet caused the world to make any change in its arrangements." Without undertaking to say what change the world should make in its arrangements if its end were to come in a few years, I may remark that my warning — such as it was — appeared in an Australian Review, and was not published in this hemisphere until a very few weeks ago (the preface to the volume is dated December, 1881, and the title-page bears the date 1882), so that the wonder rather should be how my terrible prediction comes so soon to be frightening fearful folk from their customary quietude. If it were not that his Right Reverend Lordship the Bishop of Manchester had been chiefly instrumental in calling general attention to the prediction, the world might well imagine that the scare was a well-designed puff for my new volume, in which case I might be anxious to explain that, according to the terms between myself and Messrs. Chatto & Windus, I could not possibly gain, and might conceivably lose, by the rapid sale of the work at this present time.

* I should esteem it a favour (though I think I might almost claim it as a right) if those newspapers who have spread the news of my supposed prediction, would be good enough to explain that I believe the world is more likely to last fifteen millions of years than to be destroyed in fifteen.—R. A. Proctor.

Be the cause what it may, I find that I am generally understood to have issued a prediction that, somewhere about the year 1897, this world, with all that it inherit, shall be dissolved by fervent heat. Let us see what the article referred to by the *Spectator* really says:—

In its opening paragraph, I state that views advanced respecting the comet by others, "not by fanciful theorisers, but by mathematicians of eminence, suggest the possibility, nay, even some degree of probability, that this comet may bring danger to the solar system." And I go on to say that it is that possibility which "I have to discuss." The possibility, even some degree of probability, that a comet may bring danger—this possibility suggested by the views of others, and to be discussed by me—does not, I apprehend, amount to a definite statement on my part that there is "really a very considerable chance of a catastrophe fifteen years hence, which may put an end to our earth altogether." Let us, however, examine the article further.

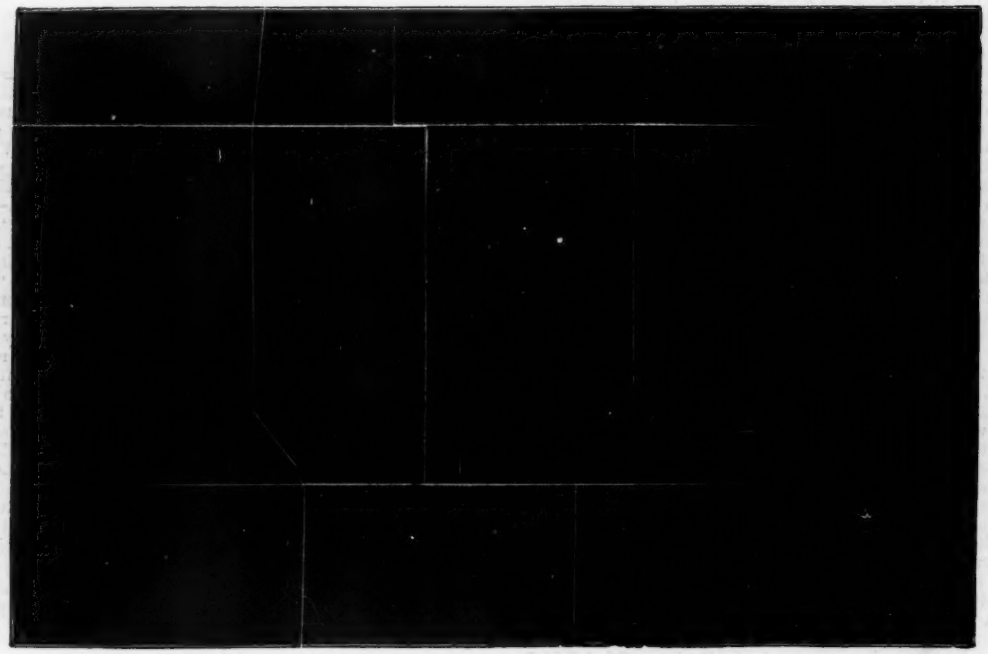


Fig. 1.—The Menacing Comet

I go on to show that the path of the comet of 1880 carried it singularly near to the sun. This, of course, is simply a scientific fact. I next explain that the observed part of the track of the comet of 1880 coincided, or nearly so, with that of the comet of 1843; but that whereas the most trustworthy calculations of the orbit of the comet of 1843 assigned a period of about 175 years, the observed period of its last circuit—if that object and the comet of 1880 are really identical—was only 37 years. This part of the inquiry is more theoretical than the former. Still, the evidence is such as to make it highly probable that the comet of 1880 really is one and the same as the comet of 1843, and that there really has been a diminution of the period of revolution from more than a hundred to less than forty years.

It is towards the close of this part of the inquiry that the anticipation of the comet's return in 1897 is referred to. As presented by the *Spectator* and the Bishop of Manchester, this might be supposed to be such a prediction

as, for instance, I made in 1868 of the epochs of the beginnings and endings of the transits of Venus in 1874 and 1882 for different parts of the earth's surface. As a matter of fact, the prediction is not mine, but Herr Marth's (quoted, and, as a quotation, given in smaller type than the rest); it is not advanced definitely, but in the following terms:—"I should not be at all surprised," says Herr Marth, "if it should turn out that this comet of 1880 is the same as the comet of 1843 and that of 1668, and that its revolution has been so much affected that possibly it may return in, say, seventeen years." (This was written in 1880.)

I go on to show that if this is so, the comet must before long be absorbed by the sun—still not naming 1897 or any other year, but speaking with due scientific caution—"after only a few circuits—possibly one or two."

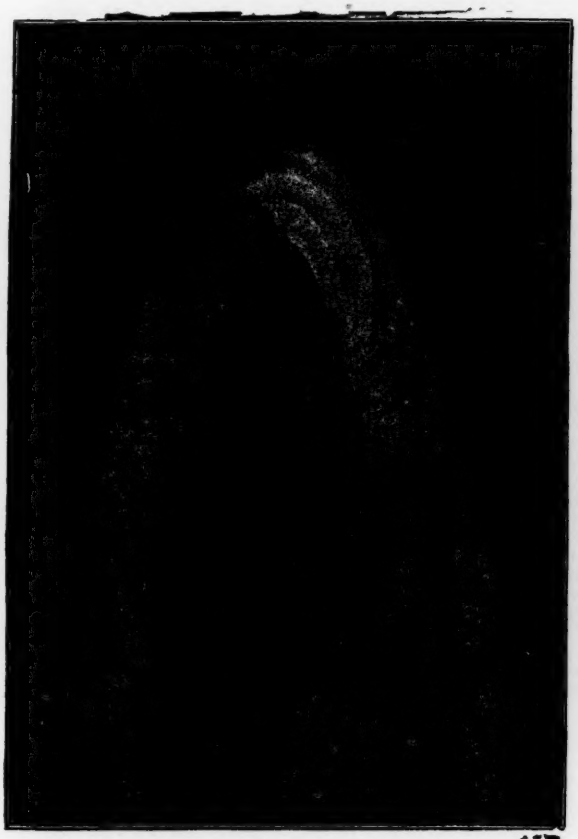


Fig. 2.—Another comet which might have been dangerous if it had gone the wrong way.

I then note the only way in which the absorption of a comet might do harm—that is, not as Newton thought, by adding fuel to the solar fires, but by the conversion of the momentum of the meteoric masses forming or following the head, into heat. I mention, in passing, my own belief that the sudden increase of splendour observed in the star T Coronæ (not τ, as has been mistakenly asserted) was due to the fall of a large comet, followed by a train of closely-aggregated meteors upon that distant sun. This I fully believe to be the most probable, if not the only available interpretation of that and similar phenomena. "Without saying," I proceed, and this seems to me the only passage in my essay which could have suggested any anxiety about the earth's future, "without saying that I consider there is absolute danger of a similar outburst in the case of our own sun, when the comet of 1843 shall be absorbed by him (a result which will, in my opinion, most certainly take place), I will go so far as to express my belief that if ever the day is to come when 'the heavens shall dissolve with fervent heat,' the cause of the catastrophe will be the downfall of some great comet on the sun." What I here consider as certain may, perhaps, have been misunderstood as the coming of such a catastrophic end; but it should be manifest that I only regard the absorption of the comet of 1843 as most certain—regarding the time as quite uncertain, and the effects as extremely problematical. I have, indeed, shown elsewhere (see "Suns in Flames," in my "Myths and Marvels of Astronomy") that there is every reason for believing that all comets of the destructive sort have long since been eliminated from the solar system. So that, as in the essay referred to by the *Spectator*, I refer back to an essay in my "Pleasant Ways in Science," in which essay I refer back to the other in "Myths and Marvels," without in either case indicating any change

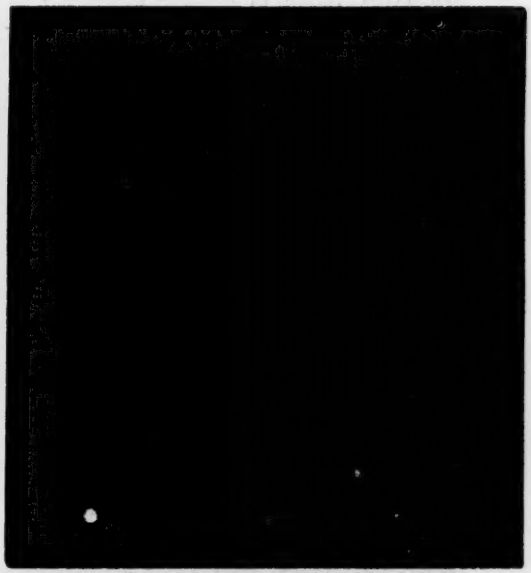


Fig. 3.—A comet which might injure a solar system, though not ours.

of view, I might fairly claim to have very definite views as to the perfect safety of the solar system, even if I had not recently pointed out, with special reference to the comet of 1843, our probable complete immunity from danger. In the *Cornhill Magazine* for December last, there is a paper, bearing my initials, on "Dangers from Comets," in which it is shown—as sundry newspaper articles have been good enough to explain in turn to myself—that if there were any real danger, save for the comet itself, we should have known it by great increase in the solar emission of heat in 1843, when the comet was checked so importantly in its career, and again in 1880, when it was subjected to another equally severe interruption of its onward course.

The article finally points out the kind of danger which in all probability would ensue if a comet of the larger sort fell into the sun. If there is anything remarkable in this part of my essay, which seems to have been regarded as the most sensational, it is its extremely cautious wording. I may go so far as to poke a little fun at myself

XUM

by saying that it is almost absurdly cautious. I point out that if there are planets circling around the sun which blazed out in Corona in May, 1866, to eight hundred times its former lustre, and if there were living creatures on them at all resembling ourselves, those creatures must most certainly have been destroyed. It takes no wizard to know this. I then go on to say that "if at any time a great comet falling directly upon the sun" (which the comet of 1843 and 1880 most certainly will *never* do) "should, by the swift rush of its meteoric components, excite the frame of the sun to a lustre far exceeding that with which he at present shines, the sudden access of lustre and of heat would prove destructive to every living creature, or, at any rate, to all the higher forms of life, upon this earth." And though I knew when I wrote this that I was making no rash prediction, I protest I never noticed until the rash predictions assigned to me by the *Spectator* and the Bishop of Manchester set me reading over my own essay, that this amounted only to an announcement of the following highly-impressive nature:—If such a comet as we have no reason to suppose actually existent (nay, every reason to consider certainly non-existent in the sun's case), should produce a degree of solar heat (which such a comet may, or may not, be capable of producing), exceeding hundreds of times the sun's present heat, and if that heat lasted but a few days, the earth's inhabitants must all perish. This very cautious announcement does not mean, I venture to point out, that fifteen years hence the comet of 1843 falling into the sun will so raise his heat that all of us will be destroyed.

I may remark that the newspaper announcement has elicited various expressions of opinion, showing the great ignorance which prevails even in these days of cheap scientific literature respecting scientific matters. Thus it has been carefully explained by some that comets are entirely vaporous, evidently in ignorance of what has been learned respecting the meteoric nature of comets; by other writers, that Lexell's comet was absorbed by Jupiter or by his satellites (which Leverrier entirely disproved); while another writer (in the *Christian World*, and followed by the *Globe*) propounds the amazing statement that the sun's heat does not travel so quickly as his light, so that even though we saw a great outburst, due to the destruction of a comet, some ninety or a hundred years would have to pass before the earth would receive the heat then generated! It would be interesting to ascertain whence this singular idea was obtained—by what strange misapprehensions of some statement in a scientific work. Of course, there is not the slightest foundation for it. The sun's heat comes to us with his light, not only travelling at the same rate, but being a part of the very same undulatory disturbance, and a considerable portion being derived from the very same waves. Some of the waves, indeed, which affect us as light affect us very little as heat, and some of the waves which affect us as heat, produce no effect which the eye can appreciate as light. But the orange and red light-waves are very active as heat-waves too, and there is not the slightest reason for supposing that the so-called dark heat-waves, which, with these, make up the total supply of solar heat, would lag many seconds behind them on the journey earthwards.

However, there is not the slightest reason to fear that the comet of 1843 and 1880 (assuming they are the same) will do any harm to the solar system when finally absorbed. It would be quite otherwise, I believe, if such a comet as that of the year 1811, Fig. 3, were to fall directly upon the sun. This, the most remarkable (in reality, though not in appearance) of all known comets (see Knowledge, No. 5, p. 86), was fortunately some 100 million miles from the sun at the time of its nearest approach to him, and can never bring the slightest trouble to the solar system. But if its course had chanced to be directed full upon the sun, the meteoric masses doubtless forming its head and train (not tail), falling in countless millions upon him at the rate of more than 300 miles per second, when they crossed his visible surface, and probably passing deep below that surface with ever and most rapidly-increasing velocity to reach his real nucleus, would have generated an intensity of heat far exceeding that which he constantly emits. The increased emission might not have lasted a month, or even a week, but it would have sufficed.

So, again, what we now know of cometic structure leads us to believe that the comet of 1858, called Donati's, whose head is shown in Fig. 2, would have proved a very dangerous visitor had its course led it directly towards the nucleus of the sun. Fortunately, the chance of any comet visiting our system from interstellar space, travelling directly towards the sun, is so small, that it may be reckoned "almost at naked nothing." As to comets already belonging to our system, if any such have orbits passing very close to the sun, so as to be checked in their career at every perihelion passage, it is clear (from the continuance of life during many hundreds of thousands of past years on the earth) that the mischief must long since have been taken out of them—unless we suppose (which is incredible) that the last perihelion passage of such a comet preceded the beginning of life on the earth.

THE EFFECTS OF TOBACCO.

By Dr. Muir Howie.

PART II.

In the present day, we can calculate with precision the exact time, to a minute fraction of a second, which is required to transmit a message from the brain to the hand or any other portion of the body; and it has been distinctly shown that it takes much longer to send such a message after the person experimented upon has taken even a small dose of a narcotic. A message which could be sent in 0·1904 of a second, required 0·2970 of a second for its performance after two glasses of hock had been administered to the subject of experiment, thus showing how much even a slight narcotic interferes with the vital action of nervous tissue. The same effect is produced by tobacco. Tobacco prevents waste of tissue, and thus enables a man who smokes to live on less food. This is considered a very strong argument in favour of the pipe; and if good food could not be obtained, it might have very great force. But plain, wholesome diet is cheap and easily procured. Moreover, "waste of tissue" is an expression which conveys an utterly false impression. There is no such thing as waste of tissue, unless the body is wearing away more rapidly than new substance can be reproduced, as in certain fevers, consumption, &c. The tissues of the body are not a fixed quantity, like the framework of a steam-engine; they are ever changing, the old wearing away to be replaced by the new. Life is a constant series of changes, and the healthier the man the more rapid, within certain limits, will be his change of tissue. You can only preserve the tissue of a healthy man by lowering his vitality; the tissues thus preserved cannot bear the strain which can be borne by those of recent manufacture, and thus the working power is diminished. An employer of labour in Liverpool, anxious for the elevation of his workmen, suggested that they might with advantage give up the use of beer and tobacco. They informed him, however, that in such a contingency their wages would not support them, so great would be their increase of appetite. But there is another side to this question, and it is, that such men would be able to do more work, and consequently earn larger wages, by discontinuing the narcotic. Men of all classes are very slow to learn that sound bodily health is the best possible investment. The human machine is very easily kept in order, but once let it get out of repair, and it is most difficult to set right. And it can only be kept in thorough repair when every joint, muscle, and nerve is maintained in a condition of persistent activity. I do not mean that a man should always be engaged in exercising his various tissues and organs in order to preserve health; but I do maintain that every tissue should be so actively exercised that it will be compelled to employ its entire

time of so-called rest in laying up fresh stores of explosive material, and in healing up those rents which have taken place in their actual substance. In the region of nerve and muscle a man ought always to live up to his income. He can save nothing by sparing exertion, so long as he does not go beyond his income. Give your brain sufficient food and an abundant supply of oxygen, and then give it a fair amount of good hard work every day, if you wish to maintain it in a high state of healthy activity. Barristers and clergymen, who use their brains much, are the longest-lived men in the country, showing plainly that regular brain work is good for the general health as well as for the efficiency of the nervous system in particular. The muscular system must be treated in a similar manner, if you do not wish it to become subject to fatty degeneration. An unused muscle shrinks, and becomes soft and flabby, presenting an appearance of marked contrast to the brawny arm of the blacksmith. Instances of the feebleness of tissues thus preserved frequently present themselves to the notice of the surgeon. A muscle is called upon to perform a vigorous contraction, but it snaps in the effort. The heart itself is sometimes torn asunder in attempting to send an extra supply of blood to some needy limb. No man can afford to lower his general vitality for the sake of mere idle gratification. He never knows when he may require all the energy which can be stored up in his tissues. A railway accident, a runaway horse, a run to catch a train, a fall on the ice, or even a fit of coughing, may bring a life of misery or an early death to one who would have passed unscathed through them all, had he allowed his nerves and muscles to wear away in vigorous activity, instead of carefully preserving them, like smoked bacon, in the fumes of tobacco. I do not attempt to deny that all narcotics possess the power to prolong life in the absence of food. I have elsewhere referred to the case of an old woman who lived for two years on opium and gin-and-water, without any food whatever, but she might as well have been in her grave. Hers was, I would not say a living death, but rather a dead life. Some may be inclined to doubt the accuracy of this story, but such will discern a possibility of its truth when I say that a narcotic seems to produce a condition of the nervous system closely resembling that of hybernating animals. The dormouse sleeps for many weeks without any food whatever. Its tissues are then in the condition of the cook's fire when covered with ashes, and if you can produce a similar condition in the human tissues, you may attain the same result of prolonged fasting. We are apt to consider the winter sleep of the dormouse as a great waste of existence; but what can we think of a reasonable man who artificially reduces himself to a similar condition during a considerable portion of the prime of life.

Tobacco soothes the exhausted and irritable nervous system after a hard day's work, and prevents the brain worrying about difficulties that may never come.

The advocates of tobacco maintain that in this manner it gives rest to the nervous system, and thus enables it to throw off work for the time, and resume it again with renewed energy. Now the mistake which our opponents make here is, that they ignore the necessity for anything but rest. What would you think of the farmer who allowed his men an hour's rest at various intervals during the day, but who, at the same time, forbade them to take food at such times, lest the muscular movements involved in carving and mastication should interfere with their complete and absolute repose? Every cell in the body is a counterpart of the whole organism. Just as the man cannot work without eating, so the cell cannot carry on its explosive action without fresh supplies of explosive material. Now, tobacco and other narcotics not only prevent the nervous matter exhibiting energy, they also prevent it absorbing its proper food; so that the rest which it obtains by means of narcotism does not enable it to resume work with renewed energy. But more, the nervous matter is thereby rendered incapable of throwing off its own ashes, which are its most deadly poison. Just as decomposing animal matter is highly deleterious to the health of the body, so the dead portions of nervous tissue become disastrous to the life and activity of their living successors. I do not attempt to deny that the relief afforded by a narcotic is most delightful and seductive. When the merchant goes home from his office, worried by a thousand trifles, and saddled with a load of cares, his nerves are agitated and restless, and the busy wheels of life seem to spin round with unceasing velocity. How delightful it is to be able, by the magic spell of tobacco, to stop those busy wheels, and to translate himself from the pains of a commercial pandemonium into the Elysian fields of perfect bliss! I confess that tobacco does all in the way of soothing that its admirers attest; it is my duty, however, to exhibit the other side of the shield, and to proclaim that the luxurious pleasure of the pipe is physiologically so expensive that the nervous system cannot afford to indulge in it. The muscles suffer along with the nerves; for without nervous influence the muscles are unable to supply themselves with the nourishment which is carried by the blood into their very substance. If you cut the nerves leading to a muscle, that muscle will cease to retain its firmness and contractive vigour, and if you paralyse the same nerves by a narcotic, its power of contraction will be similarly diminished. Any smoker will tell you that much smoking is a hindrance to severe muscular exertion. If a man has lit his pipe, you are more likely to find him dreaming in a corner than ascending a mountain. When you observe what an amount of lounging lethargy is induced by tobacco, you scarcely require an ounce of science to account for the smaller appetite of the inveterate tobaccophile. This power of the narcotic to interfere with the nutrition of the tissues produces serious consequences on the digestive organs of those who both smoke much and eat well. The smoker is often not content to suffer any diminution of the pleasures of the table as a result of his pipe. He therefore uses a variety of agents to induce in his digestive organs an artificial appetite. He is thus led to consume a much larger amount of nutritive material than can possibly be required by narcotised tissues. This nutritive material produces injury either to the stomach or liver—very frequently to both. The stomach is burdened with more work than a smoker's stomach can perform; hence the dyspepsia so frequently accompanying the pipe. The liver is doubly burdened. Its duties in connection with the food are many. It assists to prepare nutriment for nerve and muscle, and if such nutriment *is* not required, its further duty is to break down such rejected nourishment in order that it may be more easily expelled from the system. Hence the biliousness and other effects of liver derangement so common in the smoker.

Tobacco destroys the physical conscience.

My greatest objection, as a physician, to the use of tobacco, is, that it destroys what I have ventured to call the physical conscience. The entire body is supplied with minute nerve twigs which, in the healthy man, are maintained in a highly sensitive condition. Their function is to inform the brain when any derangement is taking place in the ultimate tissues. This network of nerves occupies a similar position in relation to physical health that the conscience does in relation to the moral condition. Whenever any muscle has difficulty in contracting, a message of the fault is at once transmitted to the brain. The same occurrence takes place when the stomach has difficulty in digesting its contents, when the liver is overburdened with excess of sugar or bile, and when the brain is being overtaxed with daily toil. These messages produce great uneasiness to the subject of their influences, just as a troubled conscience does in the mind of its possessor. Now, there are two ways of avoiding the inconvenience of the physical conscience, just as there are two ways of avoiding the pangs of a smitten moral conscience. You may either do what is right, or you may lull your conscience to sleep. Tobacco enables a man to deaden his physical conscience, and thus he may go on ruining his health without knowing it, until he is beyond the hope of recovery.

HOW SPIDERS FLY.

(By Professor C. A. Young.)

I WAS very much interested, a few days ago, in hearing a friend give an acconnt of a manuscript she had seen, which was written by Jonathan Edwards when nine years old. It was an account of the behaviour of certain small New England spiders, the manner they fly through the air, and the way to see them best, by getting into the edge of a shadow, and looking toward the sun. It is neatly and carefully written, and illustrated by little drawings very nicely done. The philosophical tendencies of the young writer already appear, for his conclusion as to the "final cause" of spiders and their flying is this: the little animals are scavengers, and since, in New England, the prevailing winds are west, they are carried to the sea in their flight with whatever filth they have consumed, and so the land is cleansed.

Every one knows how, in sunny weather, the little creatures, standing on their heads, project from their spinnerets fine filaments of gossamer, which are caught by the breeze, and float off into the air, though still attached to the spider. When she perceives that the thread is long enough, and the pull of the wind sufficient, she releases her hold and flies away on her gossamer like a witch on her broomstick; by watching her chance, and letting go only when the breeze is favourable; she is carried to her desired haven. Experiments have been tried by placing the animals on a chip floated in a pail of water. So long as the air was in motion about them they were able very soon to escape from their island; but when a bell glass was placed over the pail, thus preventing air currents, they could not get from the island to the surrounding shore.

But how does it happen that, on setting out for a voyage, the spider almost invariably *ascends* with her web, and continues to

XUM

rise, until, by pulling in her thread, she reduces her floating power, and so comes down? Spider web, in and of itself, is not lighter than air; how, then, is its buoyancy to be explained?

In two ways, I think. When the sun is shining, every projecting object, like a twig or stick, absorbs heat more rapidly than the air, becomes warmer than the air, and thus acts like an independent source of heat in generating an ascending current, so that when the spider lets go her hold, she and her thread are carried up partly by the action of this current.

But this is not all; unless I am much mistaken, *the action of the sun's rays on the thread itself and its surrounding envelope of air* is the main cause of its buoyancy. Air is nearly diathermanous, or transparent to heat, so that the solar rays, in traversing it, warm it only slightly. The spider's thread is not so, but in the sunshine warms up almost instantly, heating the air in immediate contact with it: and then, although the spider thread alone is heavier than air, yet the thread and the adhering envelope of warmed and expanded air taken together, are lighter than the same bulk of the cooler air around, and thus constitute a quasi-balloon, on which the spider sails away. Of course, if this is so, the poor creatures cannot sail much on cloudy days, and I think, in fact, they do not.

I have tried a few experiments to verify the idea, and so far as they go they all confirm it. For instance, one day in the autumn of 1880, when the air was full of floating gossamer, and there was no wind blowing, I caught some of the filaments at the end of a little stick, to see how they would behave. So long as I stood in the sunshine, they streamed straight upward, tugging with almost a breaking strain; as soon as I stepped into the shadow of a building, they lost their spirit, and drooped abjectly; the moment I put them in the light again, they resumed their buoyancy. It is, of course, possible that in the shade there were local downward air currents to account for their behaviour; but once a cloud passed across the sun, and they drooped then, just as they did behind the building.

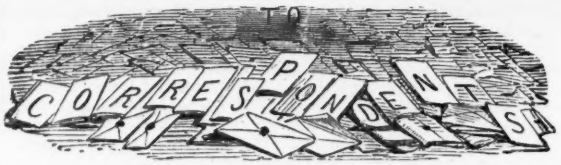
The same theory will explain the buoyancy of any minute particles of dust or smoke. So long as the sun shines, they will absorb its rays, become warmer than the air, and surround themselves with a buoyant envelope, which will carry them up if they are not too heavy in proportion to their surface. But if the air is still, and the sun obscured, they will settle down near the earth, in the way we are all familiar with in muggy weather. Of course, if there is much wind, this will mainly control their movements, and neither their buoyancy in sunshine, nor their gravity in shadow, will be particularly noticeable.—*Boston Journal of Chemistry.*

INTELLIGENCE OF DOGS.

WHILE at the University, taking my medical course, the facts I relate took place. Among other appurtenances to the department of physiological chemistry was a dog with a gastric fistula, which fistula was properly healed around a silver tube having an internal and external flange to keep it in place. The tube was stopped by a closely-fitted cork, except at such times as we needed a supply of gastric juice. The fistula caused the animal no disturbance whatever. He was well and hearty, was fed at and made his home at the medical department.

During the summer vacation, however, when the University was closed, he was transferred to the care of the surgeon, who took him to his house. During his frolics one day he jumped over a fence, striking it, and dislodged the cork in the tube. Ponto soon noticed that his food didn't seem to satisfy him, and that all he drank ran out of his stomach on the ground. His master having gone away for several days—fishing—he must needs take care of himself, so immediately on eating or drinking anything, he ran to his bed in the carriage house close by, turned on his back, and remained so for an hour or more, or until he felt satisfied that it would do for him to get up. Coaxing, threatening, and kicking by the domestics about the house, or by those whose attention was called to his actions, were alike unavailing to drive him from his place or from his supine position. Finally, some one who knew for what purposes the dog was used, examined his fistula and found the cork gone. This being restored, he was soon persuaded to go about as usual, and indicated by his actions that he understood that everything was all right. This incident can be vouched for by many reliable persons. Who will say that dogs—at least one dog—cannot reason?—F. L. Bardeen, M.D., in *Scientific American.*

Pond's Extract is a certain cure for Rheumatism and Gout.
Pond's Extract is a certain cure for Hæmorrhoids.
Pond's Extract is a certain cure for Neuralgic pains.
Pond's Extract will heal Burns and Wounds.
Pond's Extract will cure Sprains and Bruises.
Sold by all Chemists. Get the genuine. [Advt



Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

OPTICAL ILLUSION.—CAT'S EYE TIMEPIECE.

[279]—In the long string of optical illusions which have appeared from week to week in your esteemed paper, I am surprised the following has not been mentioned:—Fix an ordinary fork in the wall, and on the handle balance a small cork. Having shut the right eye, walk towards the cork and endeavour to knock it off with the little finger. It is very seldom that the cork is displaced upon first trial.

Your account of the remarkable ingenuity of the Chinese in calculation reminds me of the peculiar manner in which they are able to approximately tell the time, no matter whether the day is cloudy or dull. They will run to the nearest cat, open her eyes, if they are not already open, and will at once inform you, with a certain amount of accuracy, what time it may be; all depending, of course, upon the contraction of the iris or the size of the aperture of the pupil of the eye. What I cannot understand in connection with this process is, why the clouds in interrupting the sun's light have no effect upon the cat's eye? But I suppose the Celestial land knows not what fogs and mists are, and therefore we should not be able to avail ourselves of the advantage of feline clocks here. Even if it were so, I question whether pussy would submit with such grace as the cats in the land of the pigtail seem to, to an operation which must of necessity be far from agreeable to her.—Yours, &c.,

Ernest J. Wernham.

WEATHER FORECAST.

[280]—I noticed in one of the numbers of your most valuable publication, viz., Knowledge, some remarks anent the constant inaccuracies in the weather forecasts of our Meteorological Office, and impugning the utility of them and the cost to the nation, out of all proportion to the benefit derived. As regards our own district, viz., North Wales, we are coupled with Lancashire and N.W. England, some ninety miles off, notably one of the wettest districts in England, and not one in twenty of the forecasts is correct as applied to us. I tested them for fourteen days, and not one was correct, which I forwarded, in a tabulated form, to the office, giving on one side their own forecasts and on the other the actual weather we had experienced, and stated my opinion of the uselessness of such forecasts and the injustice of tacking us on to a district so far away; in due course I received a reply from the secretary, which I am sorry I destroyed, otherwise you should have seen

the lame excuses he made for the inaccuracies complained of; but it amounted to this:—"That if I could tell them how to cast the weather for every subdivision of the Kingdom, I should be very clever, as of course the climate varied in different districts from local causes, such as mountains, &c., and it was left to each district to modify the forecasts so as to suit their different localities"; but if so, of what earthly use are they for reference? We all know the weather we are having in our own particular district, but very often we want to see what kind of weather it is where we want to go to; better, I said to the secretary, leave us out altogether, or in justice to the district let us be classed in a district to ourselves, say "Holyhead and North Wales." The secretary, I forgot to say, took the trouble to send me a tabulated statement of what the weather had been during the fortnight alluded to at Barrow-in-Furness, and which is situated some 130 miles due north of us, and which I considered had no bearing at all upon what I had complained of, and was begging the question entirely. As you say, these daily forecasts are not to be depended upon, and are apparently only a matter of guess work, and so had better be dropped, as for reference and utility they are proved to be utterly useless.

A Constant Reader, and a Resident in the Principality.

DREAMS.

[281]—The following incident would seem to corroborate a view which I hold—namely, that some dreams have turned out, and do turn out, to be foretellings, &c.; and that science cannot possibly—at least, for the present—give any satisfactory explanation of them.

A gentleman friend of mine, whom I shall call A., knows a young lady B., whose house is situated in a crescent, about three miles from A.'s dwelling; he is pretty well acquainted with the neighbouring district, but is at a loss to make out where this crescent lay.

I would not for one moment doubt the veracity of his statements, and this is what he relates:—

"I dreamt that I was walking up some road, of which I have a faint recollection; it seemed about eventime. As I was going along, I came to a bye-road, where I saw B. walking on the right-hand side, and then enter into the third or fourth house. As soon as B had disappeared indoors, I seemed to run up to the house to have a look at it." . . . . And then he continues, "I suddenly awoke, and found myself on a chair sketching a house." A, on writing to the young lady the following day, told her his dream, and sent her the sketch at the same time.

Now, it turns out that what he dreamt really did happen, for on the previous evening B was just returning to her house, which, by-the-bye, is the fourth in the crescent (the bye-road would correspond to the crescent), and what is more strange, the sketch is such, that any one who has seen the house would immediately recognise it on the paper, and besides, there is a characteristic tree just before the house, which is reproduced in the sketch.

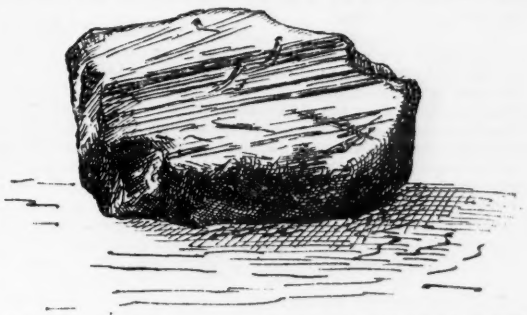
It is true that this bears the testimony of one man only; for no one but himself can tell whether he had ever seen the house before or not; he denies having seen it.

For myself, I consider the thing possible; but nevertheless wonderful and unaccountable. Perhaps one of your readers may furnish a clue to the explanation of a fact which puts me in mind of the story of Bach, the musician.—Yours, &c., Rêveur.

ICE AGE IN BRITAIN.

[282]—There are many proofs of the existence of an ice age (Query 192) over northern Europe, England as far south as the Thames, Scotland, and Ireland. The undulating outlines of the smaller hills, and the lower parts of the great ones, show the smoothing action of vast sheets of ice moving slowly over the land, and grinding down minor irregularities and abrupt eminences which came in their way, out of the *débris* of which was formed the thick tenacious clay usually found a few feet below the surface in the valleys and plains. When examined, this clay is seen to contain stones varying in weight from a few ounces to several tons. Most of these stones are subangular, *i.e.*, have their sharp edges and angles worn down and blunted, while upon their surfaces, which are more or less roughly polished and worn by friction, are scratches, some so fine as to be seen only with a magnifying glass, others being deep grooves. When such stones are oblong, the scratches are mostly in the direction of greatest length.

This rubbing down and scratching has been produced by the grinding which went on between the ice and the rock-surfaces over which it moved. The rock-surfaces in many places are scratched by the stones which have been dragged over them, and these marks show the line in which the ice moved when they were made. The direction towards which the ice moved can often be ascertained by observing glaciated stones of a different formation to that on which they are found, so that if a glaciated piece of granite is seen in a limestone district, one looks for the nearest granite beds, and if the majority of scratches on the limestone rock point in that direction, it is nearly sure to be the source whence the granite fragment was



taken by the ice. These are only some of the signs of ice work. The great basins in which lie the lakes of Cumberland and Westmoreland have been, at least partially, excavated by glaciers. This drawing is from a glaciated limestone pebble, and gives a good idea of what "Arachnida" should look for. E. C. R.

AT THE NORTH POLE.

[283]—What is the aspect of the heavens, when the sky is clear, to an observer situated at the North Pole?

Assume the epoch to be noon at Greenwich on February 3, the date of the last number but one of Knowledge. The sun will be 16½° below the polar horizon; there will, therefore, be a dim twilight. The moon will be 10° above the horizon, and just past the full. Jupiter, Saturn, and Mars will hold their relative positions as with us, but will maintain day after day a nearly constant attitude equal to their declinations (Jupiter 16°, Saturn 11¼°, Mars 27°). The constellations, together with the planets and the moon, will appear to sweep round the horizon in their daily course, the moon alone showing any appearance of setting. Her altitude will decrease about 10′, or one-third of her apparent diameter, every hour, and after two or three diurnal revolutions, she will set below the polar horizon somewhere over the mountains of Greenland, if such are in view. The monotony of such a scene would be excessive. It would probably convince the most sceptical that the earth does really turn upon its axis.

Celestial observations for the verification of the position could be taken with the same, if not with greater facility, than in our latitudes, but they would have some peculiarities. For instance, there being no time of day at the pole, Greenwich time alone would be kept, the determination of which by the moon's distance from certain stars or planets, would be the first step. Or, if the moon happened to be below the horizon, the eclipses of Jupiter's satellites could be observed for the same purpose. The calculations would be more or less simplified by reason of the assumed latitude being 90°, and one element of all other positions, namely, longitude, would no longer exist.

With no meridian of the place—or, rather, with an infinite number of meridians—there could be no such thing as a transit observation. The mariner, accustomed in temperate climates to observe the altitudes of celestial bodies at their culmination, would here wait in vain for them to "dip" that he might make his ship-time noon and find his latitude. At the Pole he would never make the time noon, but he would find his latitude notwithstanding.

In the utter confusion of the north, south, east, and west points of the horizon, no meaning could attach to the term "variation of the compass." For, although the compass at the Pole should not lose altogether its directive force, since the magnetic Pole is at some distance from the true Pole, yet, in the face of the fact that every line that could be drawn from the Pole would be in a direction due south, what could we make of the variation? The direction of the magnetic Pole given by the compass could only be called magnetic north, but with no direction that could be called true north, and with an infinite number of directions true south, it would be as absurd to speak of the variation of the compass as of the longitude of the Pole. A. N. S.

SPECTRAL LINES.—OPTICAL ILLUSION.

[284]—It may interest amateurs like myself to know that the scrapings of a galvanic battery—decomposed zinc, copper, salt, &c.—when put on to red embers of a wood fire, give very pretty

lines (Eb FG, &c., nearly a dozen) in a McClean's star spectroscope. Perhaps KNOWLEDGE would help some of us beginners by giving occasionally a few examples how certain lines may be easily produced without the aid of a Bunsen burner, as we do not all live in gas-consuming districts.

If A. T. C., page 230, will prick four pin-holes, he will see four inverted pins, each pin will appear in a circle of its own, which overlaps the one next to it; and if he will hold the piece of card the other way before and look at the square hole through one of the pin-holes, he will see the square lengthened outwards, and divided into three sections, the lightest in the middle. Two pins will seem to be in the parallelogram; half of each pin will appear much darker than the other half. Is the appearance mainly due to interference? C. R. T.

Queries.

[248]—Would any of your readers kindly help me in the following difficulty? I have a good many fossils from the Barton clay, London clay, and other similar formations. Some of these, viz., shells, are beginning to crumble, others, chiefly vegetable remains, have broken out into crystals, and some have after this disappeared into powder. I have also had impressions of leaves, &c., but these hardly lasted a week. Would some fellow geologist kindly inform me of some means of preserving these fossils from decay, as otherwise it seems a life's labour almost to form anything like a collection; and also what are the blackish metallic nodules one finds in the London clay; and are the forms it sometimes takes casts of vegetable remains, as they are very similar in appearance? And, further, are any fossils to be found in the brick earth, such as lies around West Drayton? and, if so, what kind and whereabouts in the formation is the best place to look for them?—Lepidodendron.

[249]—Abstract Reasoning.—Can any readers of KNOWLEDGE give me (*a*) a scientific definition or absolute test for abstract reasoning? (*b*) Is objective and subjective reasoning the same in kind and differing only in degree? (*c*) Are the two possessed by one and the same individual at the same time? (*d*) Does the possession of the former constitute the individual a being of the lower thinking orders, and does the possession of the latter constitute him a being of the higher thinking orders?—W. L. Abbott.

[250]—Spectrum of Actinium.—Has anything as yet appeared in print with regard to its spectroscopic behaviour?—Mabel W. Laing.

[251]—Encrinites.—Will some of your geological readers kindly give their views as to the origin and formation of the "Encrinus Liliiformis," a characteristic fossil of the Muschelkalk period. In what other strata is it also found?—Wilfred.

[252]—Sugar Analysis.—(1) Is there a chemical test for distinguishing beet-root from cane sugar? (2) In a given sample of a mixed sugar, could the relative proportions of each be estimated? (3) What is the best book relating to the subject?—P. W. K.

[253]—Natural History, &c.—Will you kindly state the best work on natural history, conchology, entomology, and meteorology, suitable for reference, and work in all parts of the world?—H. B. H.

[254]—Vines.—If the grape in its native condition is of a purple hue, can anybody inform me how the white grape has been produced?—M. E.

[255]—Is there any known means of restoring to well-coloured old prints of flowers, the original red colour which has become black through age? Deoxidation seems the thing wanted.—X. K.

[256]—Paraffin.—Is there any simple method of ascertaining the flash-point of paraffin or petroleum oils?—G. F. S. Call.

Replies to Queries.

[219]—The Atomic Theory.—For an account of the atomic theory, see the same by Ad. Wurtz; translated by E. Cleminshaw. C. Kegan, Paul, & Co., publishers. Watt's dictionary will give further information, as will the lecture "on the unit weight and mode of constitution of compounds," delivered by Professor Odling before the Chemical Society, Feb. 2, which will shorly be published in the Society's Journal, and also the *Chemical News*. There are tables for the solubility of salts, but do not know of any theory for the insolubility of certain substances in certain fluids.—Technical Chemist.

[220]—Chemical Analysis.—The following are some of the best text-books:—"Valentine's Qualitative Chemical Analysis," price 7*s.* 6*d.*; "Fresenius' Qualitative Analysis," 12*s.* 6*d.*; also "Quantitative Analysis," 15*s.*; "Sutton's Handbook of Volumetric Analysis," 15*s.*, all published by Messrs. Churchill.—Technical Chemist.

[221]—Schwegler's "History of Philosophy" is undoubtedly the best. James Hutchison Stirling's translation is published by Edmonston & Co., Edinburgh, price 6*s.*—G. A. Neucomin.

[223]—Aniline Dyes.—Ernest L. R. would find a short account of the aniline dyes in Watt's Dictionary, under Phenylamines. For more exhaustive article, see "Chemistry of the Arts and Manufactures," "Aniline and Aniline Dyes," page 204.—Technical Chemist.

[224]—Electrical.—The "rubber" for a cylindrical machine may be made by attaching a piece of leather to a piece of wood the required size, and stuffing it with horsehair; then rub on some sodium amalgam. It is not necessary to varnish the cylinder, though it is an advantage, in so far as it helps to keep moisture from condensing on it.—Amateur.

[224]—Electricity.—"A Greenock Student" will find the directions he requires in Chambers's "Electricity," published at 1*s.* (and to be obtained from any bookseller), which would be much better than a reply through the Query column of your paper, as it gives drawings. It is not necessary that the cylinder be covered with shellac varnish. Indeed, I never heard of that being done before.—J. M. C.

[233]—Biological—Physically, the difference between ape and man is much greater than that between man and man. But mentally, not. The range of variation in the capacity of the brain-case of man (healthy adult) is between 50 and 110 cubic in.; the difference between the gorilla brain-case and the lowest human is only 13 cubic in., *i.e.*, between 37 and 50 cubic in. (2.) The thyroid gland in the higher vertebrates has, by the researches of Mr. Balfour, been recognised as the rudiment of an organ called the endostyle, which occurs in the lowest group of the vertebrata, the Tunicata. (See p. 597, "Huxley's Invertebrata.") This organ takes the form of a longitudinal groove lying on the floor of the pharynx. Its function appears to be to secrete a kind of mucus which assists the process of swallowing food, though this latter point is open to question. In the floor of the mouth of vertebrate embryos, there appears a similar groove, which ultimately developes into the thyroid gland. (3.) Darwin in the "Descent of Man," quotes several instances of human beings with projecting caudal vertebræ, and I may say further that I know an individual now living who has such a tail. There have been many accounts published of races of men so adorned existing in regions sufficiently remote to prevent speedy refutation; but, so far as I know, they all lack corroboration. Herodotus mentions a tribe of Central Africans with tails, but not having access to his work just now, I cannot give details. I am indisposed to accept any of these statements, as it would be directly contrary to the theory of evolution to suppose that a useless structure should reappear and persist in a race after it had once been lost, and man's nearest allies among the apes have no tails. (4.) The Neanderthal skull. (*a.*) The brain capacity cannot be definitely ascertained, as only the roof of the cranium is preserved; but it is very small, probably below 40 cubic inches. (*b.*) The fragment of skull is remarkable for immense bony ridges projecting over the eye-orbits, of apparently nearly half an inch, and suggesting at once a comparison with those of our "poor relations," the gorillas. The facial angle cannot be ascertained. The back part of the human head where the muscles of the neck are attached, is marked by a slight ridge, called the "Lambdoidal crest." Now, the back of the head will be found to project considerably behind this point in the skull of an European, while in the lowest modern type (the Australian) it ascends perpendicularly to some height, but in the Neanderthal skull it slopes directly upwards and forwards, thus greatly diminishing its capacity. In addition to this, the brain-case is very shallow, as the following measurements will show:—A line drawn from the "Lambdoidal crest" to the front of the brow ridge gives a length of 7$\frac{3}{4}$ in., against an extreme length of 7$\frac{3}{4}$ in. in an average European skull. The height of the Neanderthal skull above the line indicated is 3$\frac{1}{2}$ in., while that of the European is 4$\frac{1}{2}$ in. Extreme width of Neanderthal skull 5$\frac{1}{8}$ in., European 5$\frac{1}{4}$ in. The length of the Neanderthal skull is apt to mislead, as the great brow ridges are not excavated to receive the brain. Professor Huxley remarks that this is the lowest type of human skull that has been discovered. The skull was found in what is called a Cave Breccia, and belongs to the so-called "Palæolithic period," which, however, is so ill-defined as to give a very indifferent idea of its exact age, though we must certainly regard it as being tens of thousands of years old.—Old Fossil.

[239]—Besser, "Primitiæ Floræ Galiciæ," Paris, 1800; Brotero, "Flora Lusitanica," Madrid, 1804; Picot de la Peyrouse, "Flore des Pyrénées," Lyons, 1793-1802. All in the B. M. Library.—H. C. F., B.Sc.

[250] "C. E. H.," and [198] "J. H. B."—Trichinæ.—The eating

of raw, trichiniferous pork is the chief cause of the propagation of the entozoon to man; but the parasite is not easily killed, even by cooking or salting. A temperature of 144° to 155° Fahr. kills the free trichinæ, but those encapsuled demand a greater heat. (Fiedler.)—During cooking, a temperature which will coagulate albumen (150° to 155° Fahr.), renders the trichinæ incapable of propagation or destroys them. As a practical rule, it may be said that if the interior of a piece of boiled or roast pork retains much of the blood-red colour of uncooked meat, the temperature has not been higher than 131° Fahr., and there is still danger. Hot smoking, when thoroughly done, does destroy them (Leuckart); but the common kinds of smoking, when the heat is often low, do not touch the trichinæ (Kuchenmeister).—CHAS. BOYLE, M.B.

[275]—ASBESTOS PAINT AND THE SAFETY LAMP.—The principle of the Davy lamp is that the heat of the ignited gas within the lamp is absorbed by the wire gauze before the incandescent particles can pass through it. If you were to coat it, as you say, with a non-conductor, it would at once lose this power of conducting heat away from the flame. The particles of gas would pass through the meshes before they were cooled below flashing point, and an explosion would ensue. No. What you want is, if possible, to improve the conducting power of the wire gauze, and also to increase its capability of radiating heat.—C. H. WINGFIELD.

Answers to Correspondents.

**** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

U. G. There is a misprint, no doubt. Hydrogen, not oxygen, must be meant, when it is said that 100 cubic inches weigh 2.14 gr.; or else for 2·14 read 32·14, though this is not quite right, it would be about 34·42.—W. H. ROSSER explains that the top-gallant forecastle is that part of the forecastle where they work the anchors, and where there is generally a windlass; also, that when the royal mast is a fitted one, there are top-gallant cross-trees. Thanks. We had not known.—E. F. B. HARSTON says sailors mean simply "very high" by top-gallant, as top-gallant head-gear—a woman's bonnet.—HERBERT WEIGHTMAN. In line 8 from bottom of 1st col., p. 307, and in line preceding the final result, the *v* in denominator of second fraction within brackets should be deleted. The mistake arose in making clean copy for printers.—WONDERFUL PHENOMENON. The calculating boy himself in that case (G. P. Bidder) explained how he worked, but, of course, not why he could work so quickly.—H. L. says, readers who want their papers cut will want next to have them read aloud. He compares them to the sturdy beggar, who, having been given a penny to get bread, said: "It will be very dry; you might give us a drop of beer to wash it down."—C. J. C. Not Lord Rosse, but earlier astronomers proved moon uninhabitable.—A. R. BROOKS. I should say, invest about £4 in a good achromatic object-glass, £1 in suitable eye-glasses, and fit them into tubes, which you could make yourself after the manner shown at p. 275. —PHŒNIX. We must not trench on the department of our medical contemporaries. We should be flooded by replies, among which some might be untrustworthy, and a few unintentionally mischievous.—JAS. A. GEE. Read Darwin on the "Formation of Vegetable Mould through the Action of Worms."—LEWIS J. COLES. Thanks.—E. MALAN. Question would involve four or five pages of replies.—STUART MURRAY. Could you not say that shorter?—F. B. HOLT. Kindly make a neat query.—T. S. Unfortunately for your position, phrenological theories have been disproved, not neglected, by science.—C. GRIMSHAW. In American houses, none of the suggested effects are noticed. As for the open fire, with its draughty ventilation, it is a disgrace to our civilisation. After enjoying the comfort of well-warmed houses in America, with a winter temperature often falling to 20 or 30 degrees below zero, I feel ashamed when I think that in England a winter in which the thermometer falls 4 or 5 degrees below zero leaves us shivering hopelessly (half roasted on one side), even in houses intended for the rich (and at thrice the cost for fuel).—VACNOL. Fear Mr. Williams cannot tell us why fleas or their flatter allies prefer some folk to others, or why some feel their bites more than others.—W. G. WOOLCOMBE. Should say that with your mathematical knowledge, Herschel's "Outlines" would be far more interesting than Guillemin's "Heavens." I have revised the latter book; but, written as it originally was by a non-mathematician, revising can be but patchwork. After reading his carefully illustrated explanation of the way in which one meteor-stream could explain both the November and August showers, a mathematician puts down the work, knowing no original opinion by a writer who could make such a mistake can be of any value. An ordinary mistake proves nothing; but such a mistake as this carefully wrought out in details can only be interpreted in one way. Yet the descriptive portions of the work are very good.—MAJOR JAS. CUMMINGS would like to know where he could purchase such a blowpipe as Lieut.-Col. Ross describes, and wishes for further information and illustrations respecting the apparatus described in Lesson 2.—SCIENTIA CUM LEGIBUS. Believe it has been shown that scents may be emitted for very long periods without appreciable loss of substance by the scent-emitting substance.—E. TAYLOR wants name of a work on wild flowers and plants, with descriptions enabling beginner to distinguish them, to cost about 2s. 6d.—NOTES UNSIGNED received respecting moisture in air, effects of tobacco, natural philosophy, and the atomic theory.—GORGON. Whether nose or ears can be changed in form by reiterated daily compressing is hardly a question suitable to these pages. Try the *Lancet* (the paper, not the instrument), or the *Medical Press and Times*. Should say the story about fall of manna in 1846, at Yenishebir, must be a canard.—W. A. C. It is unfortunate, but matters seem so arranged that unless some animals die, many human beings must perish.—TEREDO. Could the sides of an ancient river valley have the required flatness? The natural interpretation is, that the terraces are parts of sea beaches which have been displaced later from horizontality.—J. RAE. F.R.A.S. has explained his meaning.—CHAS. BOYCE, M.B., explains that opium only assists the action of purgatives (in cases of lead-poisoning) by relaxing the contraction of the intestines, but does not itself act as a purgative.—COMET. Yes: comets obey gravity. The size and mass of a celestial body in no way affect its motion; a pin's head (or a pin) sent off with the right velocity would travel in an orbit a million times larger than the earth's, with as perfect steadiness as the most massive planet.—T. R. A. Fear cannot at present find space for articles on mesmerism, though am very certain you would treat the subject from a scientific standpoint.—F. F. Question too vague. Besides, it is not fair to ask me what books I recommend on subjects upon which I have written myself.—S. S. wants Mr. Mattieu Williams to tell him how to warm a room 13 × 13 ft., which has no fireplace, and in the walls of which no holes can be made for stove-pipes.—STUDENT. Story about inherited kleptomania in a dog rather too long for us.—T. R. ALLINSON. Thanks; but questions already answered. Your replies not numbered.—CHARLES DAWE. The question whether snakes swallow their young in time of danger is rather well worn. The usual opinion of naturalists is that there is no foundation for the idea. The story you mention about the young who had eaten their way out, after being swallowed, seems very hard to take in. Let us hope it is not true.—F. H. S. No more room for magic squares. —ECLECTICUS thinks W. S. C.'s reply to his letter, p. 202, too vague to be of use. We have not space to repeat the question.—SIMPLEX. Your account of Bell's system of shorthand too abstract for our readers, and too long; we wanted only a few simple illustrations of its characteristics, as compared with those of Pitman's.—J. F. S. Thanks. Wish we could do more that way. The correspondence stops the way. Fear can find no space for letter already printed.—A NEW READER. Thanks; but new subjects of correspondence leave no room for the old ones you discuss.—W. H. K. SOAMES. You go a little beyond what I had thought possible in the line you take. When, first, scientific discoveries seemed difficult to reconcile with certain passages in the book you mention, men of your way of thinking said simply the earth is not a globe, it does not move round the sun, and so forth, because this book says differently; we want no other evidence: so they rebuked men of science for teaching such things, and told them to be still. When, however, men of science had demonstrated the soundness of their views in those matters, your friends took another line. "These facts," they said, "are right enough, and the account in the book, rightly understood, agrees perfectly well with them." So they rebuked men of science for saying that the facts did not agree with the book account, and told them again to be still, as not knowing how to interpret the book. Men of science had *not* said what they were rebuked for saying; but that was a detail. It is, however, a new thing—so far as I know—to take your line; and to tell men of science that they are *bound* to show that the account in the book is incorrect before they indicate

XUM

results which you consider inconsistent with that account. Everything you say about the origin of man might have been said with equal force about the Copernican Theory three centuries ago. Some of it *was* said. The theory was described as obviously contrary to the plain teachings of the book. But no one, so far as I know, went so far as you do, and asked that the followers of Copernicus should begin by *showing* the book "to be untrue." For my own part, I should think it was much easier to reconcile (as it is absurdly called) the modern scientific belief of man's origin with the account given in the book you mention than the Copernican Theory or the results of geological research; as men of science are far better able to inquire into the truth of the scientific theory than into the question of that book's authority, I can see no reason for urging them across the line which separates science from religion. Consider a little that very clever (but, I suspect, generally misunderstood) saying of Disraeli's, "Religion begins where science ends,"—this, with a very slight change, may be taken as "our mortar what we sticks to" in this matter:—Where dogmatic religion begins Knowledge ends. I say all this with no more objection to religious attacks on science, than to scientific attacks on religion; I object equally to both. But just think where the science of our day would be, if, before starting any theory, men of science had had to show that the interpretation put by theologians on certain passages in religious works was incorrect.—J. R. You ask when the Greater Light was called the Sun, and refer to Genesis, ch. xv. v. 12; but that account says only when it was called by another name. The present name, or form of the name, is not quite so old. As a matter of curiosity I should like to know why you wish for an answer to this question. It must be of interest to you, as you have been asking it for years: but why? What *can* it matter?—W. J. Abbott. Many thanks; if Knowledge had many such friends as you we could afford to give what hitherto only much more expensive papers could present to their readers. Geological matters will soon have their turn. Do you not think the quicksilver story—instructive though it doubtless is—better suited for a more medical journal than ours?—W. Middleton suggests that the sea-blue bird of March may be the fieldfare (see Goldsmith's "Animated Nature," vol. ii., p. 140).—J. P. Sandlands. Nay, the article on Found Links simply presents a certain part of the evidence for what it is worth; the evidence for the development of one form of creature from another form is one thing, the indication of such links as the development theory requires is another. Dr. Wilson's argument is complete, so far as it goes; but one and the same argument cannot answer all objections. I can only infer from your remarks about luck in its relation to religion, that I should not discuss the mathematical theory of probabilities, because it hurts your religious feelings to be told that, so far as observation extends, the laws of probabilities and of averages are never departed from. That would be as unreasonable as to urge that the scientific discussion of the laws of chance does not take into account the possible influence of earnest prayer on the observed result in the case of any particular person. A and B draw tickets in a lottery, prize £1,000, number of tickets 1,000; and science says the chance of each is worth just £1. But J. P. Sandlands interferes. "Do not trespass on forbidden ground," he says: "whether you mean it or not, the inference from that statement is against religious truth. If you make a statement, and say this is all we know, and speak as an authority, does it not follow that there is nothing more? If you examine the statement you will find it ignores the interaction of scientific and religious truth." A, you imply, may be favoured for reasons which religion suggests and can explain, and science, in making no mention of this, to all intents and purposes suggests that there is no such influence from without. It may seem so to you. I cannot take your view. As I have already said, the mathematical discussion of chance seems to me to have no more to do with religion than religion has to do with experiments in chemistry or physics.—Q. Many thanks. The mistake was discovered soon after the book was published. Many errors in the treatise were such as I could not possibly have avoided—accents not showing which had shown in proof, and the like. The work was very laboriously corrected, but was unfortunately in the hands of printers of very little mathematical experience.—F. W. F. Problem later.—F. S. The distance you require—the sagitta (say s)—is given in terms of the chord ($2c$), and the radius (r) by the following formula:— $s = r - \sqrt{r^2 - c^2}$. When r is very large compared with c, we have approximately $2rs = c^2$, or $s = c^2 + 2r$.—C. H. Wingfield. Thanks: but the experiments to be found in elementary treatises on pneumatics and chemistry. We must not occupy space telling readers what so many know.—F. G. Barkas. As to my supposed prediction—not quite. I would be glad to see the article you mention. There would be nothing to prevent its appearance here, if the reasoning seemed valid. I think it very unlikely I could lecture at Newcastle, or anywhere, next season. Thanks, equally.

Letters Received.

H. Mc W., R. Haveley, Gl. Trevelyan, W. A. C., A Fellow of the Chemical Society, N., T. W., A. E. S., Argo, J. Acrington, Berlase, J. Twaite, L. Parmenter, A. K. Ld., Bronte, J. Harmens, L. Tuthuil, M. Amb. K., Ecce Signum, J. Harvey, M. N. S., F. K. G., L. Purtneus (?), F. R. G. S. (No. 1), Hakluyt, A. L., T. P. R., F. R. G. S. (No. 2), (Birmingham.)

Notes on Art and Science.

We are informed that Messrs. Barraud & Lunds, the patentees of the System of Time Signalling and Clock Synchronizing so largely adopted in London, have applied to the Postal Telegraph Department for an extension of their Time Exchange to Telephone purposes. For some time they have been experimenting with the object of utilising their existing system for telephoning, which has resulted in a perfect success, and they now propose to formally thus duplicate and extend their system, by which every telephone subscriber would be able to receive an hourly Greenwich time current by simply hanging his receiver on a hook in his telephone, marked "Time Signal," without any interference with the speaking power of the instruments. The proposals are under consideration at St. Martin's-le-Grand, where so beneficial an arrangement can hardly fail to receive due attention.

Manufacture of Gas from Wood.—It may not be known to all the readers of Knowledge that carbonic oxide (CO), the manufacture of which "F.C.S." refers to in the number for Jan. 20, is extremely injurious to life, if inhaled in any quantity. Carbonic dioxide (CO_2) is deleterious because it takes the place that should be occupied by oxygen; whereas carbonic oxide is a distinct poison, causing death in a very short time; therefore, should any attempt its manufacture, they should be very careful to allow none to escape into the room.—F. D. H.

Glycerine Leather Polish.—Mix intimately together 3 or 4 lb. lampblack and ½ lb. of burned bones with 5 lb. glycerine and 5 lb. syrup. Then gently warm 2¾ oz. of gutta-percha in an iron or copper kettle until it flows easily; then add 10 oz. of olive oil, and, when completely dissolved, 1 oz. stearine. This solution, while still warm, is poured into the former and well mixed. Then add 5 oz. gum senegal, dissolved in 1½ lb. water, and ½ oz. lavender or other oil to flavour it. For use, it is diluted with 3 or 4 parts of water. It is said to give a fine polish, is free from acid, and the glycerine keeps the leather soft and pliable.—*Polytech Notizblatt.*

An influential company, having Messrs. Marshall Jewell, Charles E. Mitchell, Morris F. Tyler, and Robt. Wheeler Wilson on their board, and with all their capital privately subscribed, has just been formed in America, to be called "The Standard Time Co." Having carefully examined into the various systems of synchronising clocks in use both in America and Europe, the one in use in London, and known as Barraud & Lunds', has been adopted by them; a gentleman was specially sent over to England to the patentees, negotiations were at once opened, communications cabled, and the whole concession concluded in a few days. The arrangements include the whole of the American Continent, as well as the United States, and an effort will be made to bring about a concerted system of time signalling over the whole of the States; local affiliated companies are to be formed, and we shall probably soon hear of our transatlantic cousins having once more excelled us in an extensive exchange and utilisation of time signals, as they have in their telephone exchanges.

Our Mathematical Column.

T. R.—Many thanks for your algebraical proof of the relation involved in our geometrical solution of the Messenger problem. "No Analyst," however, in his personal communication, said he could follow a proof referring to hyperbolic area, which would, he supposed, be involved, and referred to a problem which it appears is given in Frost's Newton. Will give next week a geometrical proof that area of hyperbolic section is related that way to natural logarithms. Unfortunately, our mathematical space is at present somewhat limited, and we are obliged to indulge as little as possible in analytical investigations. Hope soon to begin some simple papers on the differential and integral calculus, which will somewhat widen our sphere of operations. We must notify our mathematical readers, that recently we have allowed our consideration for those who have sent problems for solution to interfere greatly

with our original plan; and that, as that plan involved a promise, we must adopt in future a different line. We cannot further find space for such problems as occur in ordinary mathematical reading, except when illustrating general principles. Our mathematical column must not degenerate into a puzzledom corner.—ED.

A correspondent (N. XL.) asks for a demonstration of a property of the conic sections to which we referred a short time back, viz., that if a sphere enclosed in a cone (like a ball in a conical cup) touches the plane of section, the point of contact between the sphere and the plane is a focus of the conic section. We have prepared three diagrams corresponding to the case of (1) ellipse, (2) hyperbola, (3) parabola, and will give these next week, with a demonstration which seems to us of interest, as probably the simplest proof connecting the fundamental property of the ellipse, parabola, and hyperbola (relation between distances from focus and directrix), and the fact that the curves possessing that property are sections of the cone.—ED.

MATHEMATICAL QUERIES.

[35]—VALUE OF LEASE.—Given, 14½ years repairing lease; rent, £15; ground-rent, £4; present rental value, £32. Required, present worth of lease to make 5 per cent. interest.—JAMES GREGG.

[36]—Can you, or any of your readers, tell me how to obtain the general term in the expansion of $(a_1 + a_2 + a_3 + \&c.)^n$, n being whole or fractional, positive or negative?—CARTESIAN.

[We should deal with the problem somewhat on this wise:—Let any expression of the form $a_m + a_{m+1} + a_{m+2} + \&c., = a_m$: also in the expansion of $(a_m + a_{m+1})^p$ take the $(r_m + 1)$ th term for general term, and put $p - r_m = r_m$. Then

$$(a_1 + a_2 + a_3 + \&c.)^n = (a_1 + a_2)^n = \Sigma \left[\frac{n(n-1)\ldots(n-r_1+1)}{1.2.3\ldots.r_1} a_1^{r_1} a_2^{r_1} \right]$$

$$(a_2)^{r_1} = (a_2 + a_3)_1^r = \Sigma \left[\frac{|\underline{r_1}}{|\underline{r_2}\,|\underline{r_2}} . a_2^{r_2} a_3^{r_2} \right]$$

$$(a_3)^{r_2} = (a_3 + a_4)^{r_2} = \Sigma \left[\frac{|\underline{r_2}}{|\underline{r_3}\,|\underline{r_3}} . a_3^{r_3} a_4^{r_3} \right]$$

$$(a_4)^{r_3} = (a_4 + a_5)^{r_3} = \Sigma \left[\frac{|\underline{r_3}}{|\underline{r_4}\,|\underline{r_4}} . a_4^{r_4} a_5^{r_4} \right]$$

&c., &c., &c.

$\therefore$ finally $(a_1 + a_2 + a_3 + \&c.)^n$

$$= \Sigma \left[\frac{n(n-1)\ldots(n-r_1+1)}{|\underline{r_2}\,|\underline{r_3}\,|\underline{r_4}\,|\underline{r_5}\ldots} a_1^{r_1} a_2^{r_2} a_3^{r_3} a_4^{r_4} \ldots \right.$$

Where r_2, r_3, r_4, &c., are positive whole numbers, and

$$n = r_1 + r_2 + r_3 + r_4 + \&c.,$$

If n is a positive whole number, we may conveniently interchange r_1 and r_1 in the first part of the process (the distinction being only introduced because if n is not a positive integer, neither is r_1). We thus obtain the convenient formula

$$(a_1 + a_2 + a_3 + \ldots.)^n = \Sigma \frac{n\,(n-1)\,..\,(n-r_1+1)}{|\underline{r_2}\,|\underline{r_3}\,|\underline{r_4}\ldots} a_1^{r_1}\, a_2^{r_2}\, a_3^{r_3} \ldots$$

$$= \Sigma \frac{|\underline{n}}{|\underline{r_1}\,|\underline{r_2}\,|\underline{r_3}\,..} a_1^{r_1}\, a_2^{r_2}\, a_3^{r_3} \ldots$$ ED.

Our Whist Column.

WHIST PROBLEM, No. 1.

In this problem B holds the following hand:—

Spades.—Ten, nine, six, five. (Trumps.)
Hearts.—Ace, Queen, four, two.
Diamonds.—Queen, six.
Clubs.—Ace, ten, eight.

And the four first tricks are as follows, the underlined card winning trick, and card below leading next:—

	A	*Y*	*B*	*Z*
1.	C 6	C Kn	C A	C 3
2.	H 9	H 5	H 2	H 10
3.	D 8	D Kn	D Q	D 4
4.	S Kn	S A	S 5	S 7

After these four tricks have been played *B* is able to place every card, supposing that all the players have followed the usual rules for play.

No one has solved this problem correctly. Fifteen solutions sent.

We note that what we have hitherto said about whist leads does not quite suffice for the solution of this problem, though it help towards it. It is necessary to supplement the rules for lead, however, with only two general rules, one for second, the other for third player, to give the solution. These are first that second player, if he has a sequence of two high cards and one small one, plays the lowest of the sequence second hand on a small card led; secondly, that third in hand plays highest if he has any card higher than (and not in sequence with) his partner's lead, and no sound finesse open to him, but otherwise plays his lowest.

First Trick.—*A* has led the lowest from four at least (it should have been noticed that the inventor of this hand did not accept the rule for penultimate lead). Since two is not in *A*'s hand, nor in *Z*'s, for *Z*'s lead third hand shows he was not signalling for trumps, and *B* has it not himself, it must lie with *Y*. But no other small card can be in *Y*'s hand, who would only play Knave, having the two, if he held Queen, Knave, two, and no more. Hence four and five lie with *Z*, and no more, for *A* must have four Clubs. Thus the Clubs were originally distributed as follows:—With *Y*, Queen, Knave, two; with *Z*, five, four, three; with *B*, Ace, ten, eight; and the rest, viz., King, nine, seven, and six with *A*.

Second Trick.—*A* has no Hearts above ten, and his play of nine shows he has none lower. Hence, *A* only holds Hearts nine. As *Y* plays the five, he does not hold the three (he had not begun a signal in first round, as *B* knows, holding Clubs ten in his own hand). Hence, Hearts three must be held by *Z*, and as he played ten, having the three, he must have the Knave, but no others. Hence, the Hearts lay originally as follows:—

With *A*, the nine; with *Z*, Knave, ten, three; with *B*, Ace, Queen, four, two; and the rest, viz., King, eight, seven, six, and five with *Y*.

Third trick. Diamonds four is the lowest of four at least. *A* has no card below the eight, hence the two and three must be with *Y*, as *A* is certainly not signalling. We know also that *A* has not five trumps, or he would have begun with one; hence, as he had originally four Clubs, one Heart, and fewer than five trumps, he must have more than three Diamonds. Since eight is his lowest and *Z* has led from four at least, *B* having Queen, six, and *Y* Knave, three, two, it follows that *Z* must have held seven, five, four, and either Ace or King, showing that *A* must have had eight, nine, ten, and either Ace or King. But *A*'s first lead shows that *A* must have the Ace and not the King, for he would not have led Clubs from six, seven, nine, King, if he had had eight, nine, ten, King of Diamonds; though, following Clay's rule, he would have led a Club if holding eight, nine, ten, Ace of Diamonds, reserving the Ace-headed long suit to get in with later. Thus the Diamonds lay originally as follow:—

With *Y*, Knave, three, two; with *A*, eight, nine, ten, Ace; with *B*, Queen, six; and the rest, viz., King, seven, five, four, with *Z*.

Fourth Trick.—*B* knows already that *A* holds four Spades; *Y*, two Spades; and *Z*, three. As *Z* plays the seven, the only cards left which can make up his remaining two are the eight, the Queen, and the King. He cannot have both Queen and King, or he would have played the Queen. He must have, then, either eight Queen or eight King. But if he had the Queen, King would lie with *A*, and *A* would not have finessed the Knave holding King, Knave, and two others. Therefore *Z* held King, eight, seven. *Y*'s other card must be a small one, and Spades were originally distributed as follows:—

Z,—King, eight, seven; *B*,—ten, nine, six, five; *Y*,—Ace, two (or three, or four); and the rest, viz., Queen, Knave, four, three (or four, two, or three, two) with *A*.

The doubt as to the actual value of the small spade in *Y*'s hand can hardly be said to affect the statement that *Z* knows the position of every card in the pack, for the two, three, and four, are in this case of practically equal value.

We would now leave our whist readers to explain why *B* led trumps fourth round, when, with his knowledge of the position of cards he might, one would say, lead his only remaining diamond, through *Z*'s King, enabling *A* to make the trick with the nine.

G. THOMPSON. *B*'s lead second trick is correct. It is unfortunate having to lead from a tenace suit; but it is better than deceiving partner. Returning partner's suit at once means, "I have no strong suit."—H. P. YARMOUTH. Your method of dealing with the problem discussed by the Editor at p. 301 (letter 259) is incorrect. Do you not see that in five cuttings, according to your method, *A* would possess fifteen chances out of thirteen, which is absurd?—GRADATION. The lead of King followed by Queen from Ace, King, Queen, &c., should certainly have been added (it is indicated at p. 259); but not "Ace followed by ten from Ace, Queen,

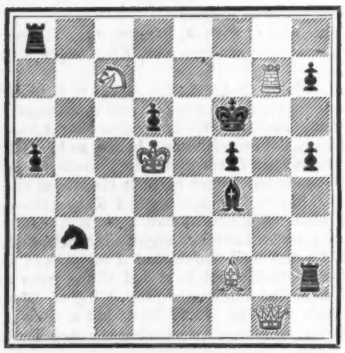
Knave, ten"; because that is not correct. "Ace, followed by nine (instead of lowest or next to lowest) from Ace, Queen, ten, nine, and others, or from Ace, Knave, ten, nine, and others" is correct in general; but hitherto, though we have touched on the play second round, we have not intended what is said about that round to be exhaustive. Special considerations come in for second round, which render special treatment necessary in its case. One would have to mention exceptions in the last two cases; for if King and Knave fell first round in the former, or King and Queen in the latter, the highest of the sequence would be the proper card to lead second round. Our leads are complete, and the learner would make the few necessary exceptions for second round if he possessed average intelligence, just as he would not, merely because of the general rule "third in hand play your highest," put his King on his partner's Queen. As to the trump leads, we should have said that from Ace, King, not more than four others, and from King, Queen, not more than four, smallest is led. We believe we wrote four in both cases, but, as you note, it is printed "five." Thanks. The chance problems later. Some of your solutions look too simple, but they may be right. May perhaps ask Editor to give these as mathematical problems. Five of Clubs.

Our Chess Column.

Problem No. 20.

By J. A. Miles.

Black.



White.

White to play and mate in four moves.

SOLUTIONS.

No. 9.—End Game, by A. J. Maas, p. 239.

1. P. to B.6. — K. to K.sq. or *A*. 2. K. takes P. — P. to Kt.5. 3. P. to B.7.(ch.) — K. to B.sq.
4. K. to B.6. — P. to Kt. 6. 5. P. to K.5. — P. to Kt.7. 6. P. to K.6. — P. Queens.
7. P. to K.7. — mate.

A. 1. — P. to Kt.5. 2. K. to B.4. — P. to Kt.6. 3. K. takes P. — P. to K.4.
4. K. to Kt.4 — K. to K.3. 5. K. to Kt.5. — K. to B.2. 6. K. to B.5. — and wins.

If White plays 1. P. takes P.(ch.), Black obtains a draw by correct play.

End Game, No. 10., p. 240.

1. Q. to B.6.(ch.) — K. to B.sq. or *A*. 2. Q. takes P.(ch.) — P takes Q. and White is stalemated.

A.—If Queen interposes perpetual ch. on R.8. and B.6.

No. 11.—Problem by Herr Gunsberg, p. 240.

1. Q. to K.R. 2. — R. takes Q. best. 2. Kt. to K.8.(ch.) — Kt. takes Kt. 3. Kt. to K.6. — mate.

Most of our correspondents gave 1. Q. to Kt.3.; but if Black replies with 1. R. to R.4.(ch.), there is no mate in two moves.

Problem No. 11, p. 259.

1. Kt. to B.5.(ch.) — K. to Q.5 or *A*. 2. B. to K.3.(ch.) — if K. takes B. 3. Q. to B.2. — mate.
if 2. — K. to B.6. 3. Q. to Q.Kt.3. — mate.
if 2. — K. to K.4. 3. Q. to Q.6. — mate.

A. 1. — K. to K.4. 2. Q. to Q.7. — K. to B.3. 3. Q. to K.6. — mate.

Problem No. 12, p. 260.

As pointed out by us, this has two solutions, viz.:—

1. Q. takes R. — B. takes Q. (best) 2. R. to K.Kt.5. — Kt. takes R. 3. Kt. takes R.P. — P. to Kt.3.
4. B. to Q.3. — mate.

or

1. Q. takes P. — Q. to K.sq. or *A*. 2. Kt. takes R.P. — Q. to K.4. 3. Q. takes B.(ch.) — Q. takes Q.
4. Kt. to B.3. — mate.

A. 1. — P. to Q.4. 2. Kt. takes R.P. — P. to Kt.4. 3. P. to Q.4. — and mates accordingly.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Edward Sargant (Problem No. 11, p. 240).—If Kt. to K.6.ch., then Kt. takes Kt. with a check, and there is no mate.

J. P.—In Problem No. 18 or No. 6, if 1. Kt. to B.5. — B. takes Kt. 2. Q. to K.6. — "B. to Q.6." then 3. B. takes P. — mate.

Squire.—Solution of No. 14 correct. We agree with you as to its merits.

J. H. Wootton.—There is no modern treatise on odds; we hope soon to publish some articles on these openings. If you give a Pawn, you must give your K.B.P.

E. C. H.—1. You can have as many Queens for as many Pawns as you can advance to the eighth row; 2. In Castling on the Queen's side, the King is put on B.sq. and the Rook on Q.sq.; 3. "Stale"-mate is a draw.

F. W. B.—Solution of No. 14 correct. It is convenient for writing down a game that is being played that the first player should play with White; but it is not compulsory.

G. Woodcock.—Your joke, directed against us at "full cock," is good; but you are an adept in the art of firing. Remove Pawn on black Q.R.2.

M. J. Harding.—We willingly grant your request, free of charge.

Henry Planck.—Solutions correct.

F. Edmonds.—Thanks for games, which shall appear. "Mephisto" and Chess Editor of Knowledge are "one" in the flesh! but "two" in the spirit.

Notice.—A gentleman would be glad to hear of another willing to play a game by correspondence.—Address, Chess Editor, Knowledge.

Contents of Knowledge *No.* 15.

	Page
The Eye and the Microscope. By Henry J. Slack, F.G.S., F.R.M.S.	311
About Fallacies. By the Editor	311
Nights with a Three-Inch Telescope. By "A Fellow of the Royal Astronomical Society." (*Illustrated*)	312
The Electric Telegraph. By W. Lynd	313
The Great Pyramid. By the Editor (*Illustrated*)	315
The Crystal Palace Electrical Exhibition. First Notice	318
Natural Rubbish Heaps. By James Geikie, LL.D., F.R.S.	318
Dr. Carpenter on Vaccination	319
The Moon's Birth by Tidal Evolution	320
The Menacing Comet	320
Correspondence:—Our Letters, Queries, and Replies—Flesh Food—The Moon and the Weather—Intra-Mercurial Planet, &c.	320-323
Queries	323
Replies to Queries	324
Answers to Correspondents	325
Letters Received	327
Notes on Art and Science	327
Our Mathematical Column	328
Our Whist Column	328
Our Chess Column	330